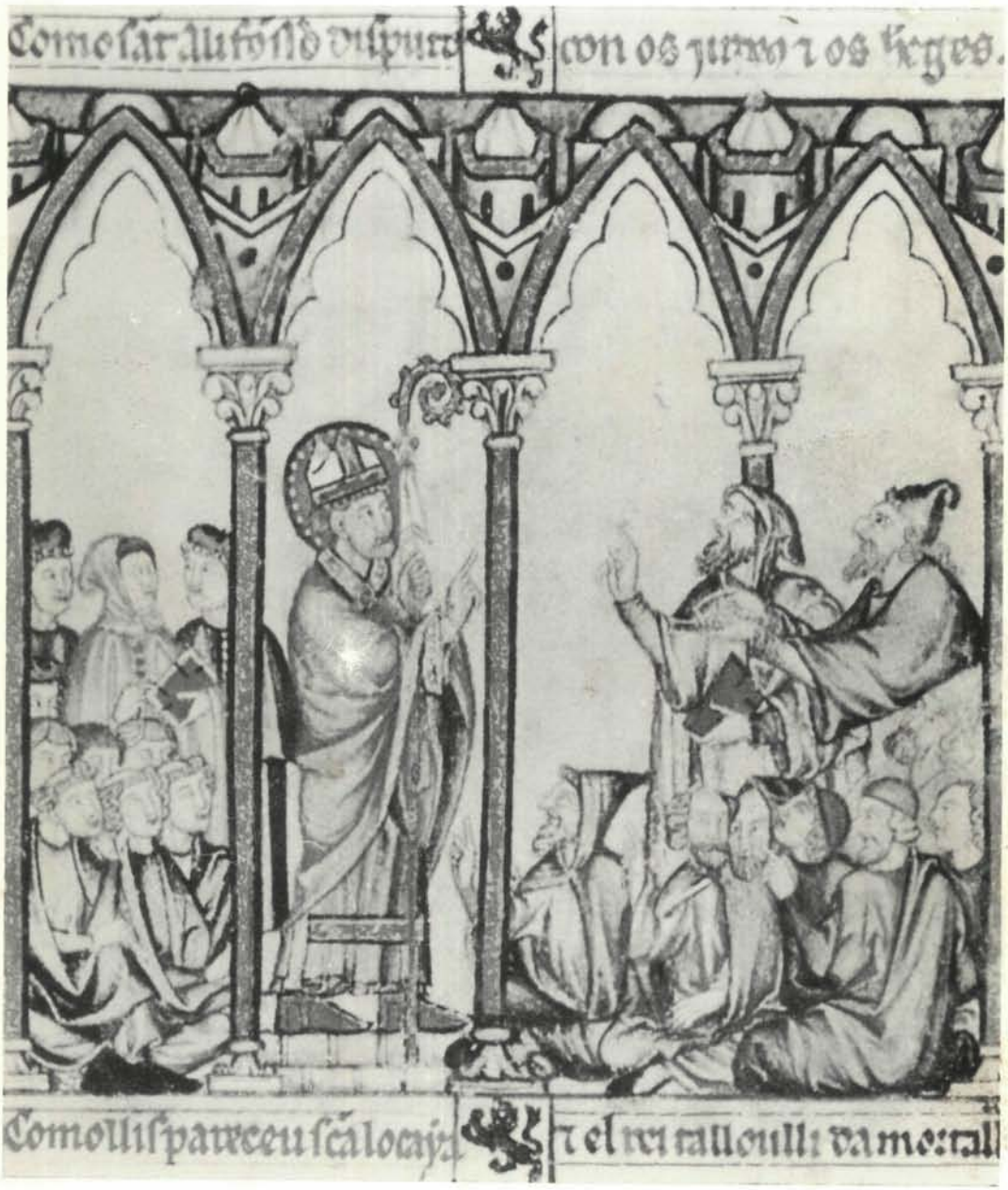
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In This Issue

Recent developments in the writing of medieval European history are the focus of this issue. The contributors demonstrate the importance of World War II in shaping historical thought in the late twentieth century. In the first article, JOHN VAN ENGEN describes how the study of religious life first entered the mainstream of medieval historiography fifty years ago and now dominates the high culture–popular culture debate. Our own shifting historical interests and confessional loyalties, he argues, obscure the centrality of "Christendom" as the customary culture of medieval Europe. To understand that culture, historians must study the ritual and legal mechanisms put in place to foster Christendom, the dynamic inherent in acting upon religious belief as well as the obligations and expectations for a population made up almost entirely of the baptized, and the means and media used to convey the contents of a book religion to an illiterate populace. The result was a total synthesis, from both above and below, which itself became the oral and customary culture of medieval Europe.

Since World War II, German medievalists have been especially interested in the nobility. JOHN B. FREED argues that the underlying premises shaping postwar German historiography—for example, the linkage of freedom with service and the belief in the nobility's right to rule—originated or became popular during the Third Reich. Since then, German interest in the nobility has remained essentially political. German medieval historians use social history to rationalize political outcomes such as Germany's late unification in the Bismarckian settlement of 1871.

The *AHR Forum* presents a modern polemic about medieval polemics. Noting the heightened acerbity of debates between Christians and Jews in the thirteenth century, DAVID BERGER proposes that aggressive Jewish critiques of Christian beliefs provoked the medieval Christian polemics against Judaism. JEREMY COHEN argues, to the contrary, that the Christians took the offensive: in the twelfth and thirteenth centuries Christian scholars failed to be objective about Jewish ideas of which they had recently become aware. GAVIN LANGMUIR broadens the perspective, bringing together both sides of this debate to note that the remarkable increase since 1945 in interest in the history of the Jews has had a considerable impact on the way we now write the history of Christianity.

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Contributors

DAVID BERGER is a professor of history at Brooklyn College and the Graduate School of the City University of New York. He studied history at Columbia University with Gerson Cohen and Salo Baron and Rabbinics at Yeshiva University with Rabbi Joseph B. Soloveitchik. Berger's *The Jewish-Christian Debate in the High Middle Ages* (1979) was awarded the John Nicholas Brown Prize for 1983 by the Medieval Academy of America, and he is currently working on the problem of exile in medieval polemic. He is editor of *The Legacy of Jewish Migration: 1881 and Its Impact* (1983) and *History and Hate: The Dimensions of Anti-Semitism* (forthcoming, 1986). Shorter studies by Berger have dealt with messianism, biblical exegesis, and other aspects of the intellectual history of the Jews.

JEREMY COHEN received his graduate training at Cornell University under the direction of Brian Tierney and Benzion Netanyahu. He subsequently taught at Cornell and now holds the Samuel and Esther Melton Chair of Jewish History at Ohio State University. The essay in this issue represents a sequel, although one that proceeds backward in time, to his *The Friars and the Jews: The Evolution of Medieval Anti-Judaism* (1982), winner of the National Jewish Book Award in 1983. Pursuing his interest in ancient and medieval cultural history, he is now writing a book on the importance of the first chapter of Genesis to Judaism and Christianity, a case study in the evolution of the biblical legacy in Western religious traditions.

JOHN B. FREED is a professor of history at Illinois State University. He received his Ph.D. from Princeton University, where he studied with Joseph R. Strayer. His principal publications include *The Friars and German Society in the Thirteenth Century* (1977) and *The Counts of Falkenstein: Noble Self-Consciousness in Twelfth-Century Germany* (1984). He is engaged in a genealogical, prosopographical, and topographical study of two hundred fifty ministerial lineages in the archdiocese of Salzburg between 1100 and 1343. In his examination of the ministerials, he is investigating such topics as age at marriage, career patterns, family strategies, social mobility, and the status of women. The provincial government of Salzburg gave him a prize for his work in 1983.

GAVIN I. LANGMUIR is a professor of history at Stanford University. His main project is a study of the nature and origin of anti-Semitism. He has published a number of articles on attitudes toward, and treatment of, Jews in the Middle Ages. His most recent publications are "Thomas of Monmouth: Detector of Ritual Murder," *Speculum* (1984), and "Historiographic Crucifixion," in Gilbert Dahan, ed., *Les Juifs au regard de l'histoire: Melanges en l'honneur de Bernhard Blumenkranz* (1985).

JOHN VAN ENGEN is an associate professor of history and director of the Medieval Institute, University of Notre Dame. He studied at the University of California, Los Angeles, under Gerhart Ladner, Lynn White, Jr., and Robert Benson and at Heidelberg under Peter Classen. He is the author of *Rupert of Deutz* (1983), and he has recently published a re-evaluation of Benedictinism in the twelfth century, "The 'Crisis of Cenobatism' Reconsidered," *Speculum* (1986). He has just completed the collection and translation of texts on a late medieval religious movement in the Lowlands, which will be published as *The Brothers and Sisters of the New Devotion* (forthcoming), to be followed by an edition of Latin texts and a broader study of devotional movements in the age of the Great Schism.

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The Christian Middle Ages as an Historiographical Problem

JOHN VAN ENGEN

THE MIDDLE AGES, THE SO-CALLED MIDDLE TIME between antiquity and the era of the early modern reformers, a full one thousand years of European history, has been subject to wild swings in interpretive emphasis, ranging from the "dark ages" described by Renaissance humanists, Protestant reformers, and enlightened philosophes to the "golden age" depicted by Restoration Romantics and neo-Scholastic Catholics.[1] But each of those outlooks still shared the common presupposition that medieval culture was essentially "Christian" or "Catholic," whether it was vilified as so much superstition or revered as so much authoritative tradition. Precisely that common assumption has come under scrutiny in the last twenty years. All the texts pored over by generations of medievalists, some historians now claim, have disclosed the views only of a minuscule clerical elite; the great mass of medieval folk lived in a "folklore" culture best likened to that observed by anthropologists in Third World countries.[2] Forms of primitive magic and not faith largely governed religious-cultural attitudes and practices.[3] This

A first draft of this essay was read at the Ninety-Ninth Annual Meeting of the American Historical Association, held in Chicago, December 27–30, 1984, at a session jointly sponsored by the Catholic Historical Association and the American Society of Church History. My thanks to Bernard McGinn of the University of Chicago for the invitation, Karl Morrison of the University of Kansas for his thoughtful commentary, and William Courtenay of the University of Wisconsin for encouragement.

[1] For a general orientation and bibliographies, see Giorgio Falco, *La polemica sul medioevo* (Turin, 1933); Wallace K. Ferguson, *The Renaissance in Historical Thought* (Boston, 1948), 113–32, 290–385; Hans R. Guggisberg, *Das europäische Mittelalter im amerikanischen Geschichtsdenken des 19. und des frühen 20. Jahrhunderts* (Basel, 1964); Jürgen Voss, *Das Mittelalter im historischen Denken Frankreichs* (Munich, 1972); and Ludovico Gatto, *Viaggi intorno al concetto di medioevo* (Rome, 1977). For newer work, see Norman Cantor, "What Has Happened in Medieval Studies?" *Humanities*, 5.5 (1984): 16–19; Karl Morrison, "Fragmentation and Unity in 'American Medievalism,'" in M. Kammen, ed., *The Past before Us* (Ithaca, N.Y., 1980), 49–77; Ovidio Capitani, *Medioevo passato prossimo: Appunti storiografici tra due guerre e molte crisi* (Bologna, 1979); and Piero Zerbi, *Il medioevo nella storiografia degli ultimi vent'anni* (Milan, 1976).

[2] Jacques Le Goff first promulgated widely this folkloric view of medieval culture. See Le Goff, *La civilisation de l'occident médiéval* (Paris, 1964), 18–19. But the notion was not entirely new. See C. S. Lewis, "Imagination and Thought in the Middle Ages," *Studies in Medieval and Renaissance Literature* (Cambridge, 1966), 41. Also see the still-earlier works mentioned in note 39, below.

[3] Keith Thomas, *Religion and the Decline of Magic* (New York, 1971): "The medieval church thus appeared as a vast reservoir of magical power" (p. 45). "The line between magic and religion is one which it is impossible to draw in many primitive societies; it is equally difficult to recognize in medieval England" (p. 50). Many other historians have pointed up widespread "magical" conceptions of religious life, but in ways that qualify Thomas's sweeping description. See, for instance, Aaron J. Gurjewitsch, *Das Weltbild des mittelalterlichen Menschen* (Munich, 1978), 352–400; Joseph-Claude Poulin, "Entre magie

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newest historiographical shift has proved so powerful that even authors kindly disposed to Catholic Christianity have summarized the state of late medieval religious life as "in broad stretches a religious consciousness that can hardly be called Christian."[4] A little background will indicate the extent of this shift.

In 1799, as the Holy Roman Empire of the German Nation verged on collapse, a petty nobleman known as Novalis drafted an influential essay, "Christendom or Europe," recalling "that beautiful and brilliant time when Europe was a single Christian land." Just three years later, from exile in London, Chateaubriand vigorously defended the medieval *Genius of Christianity*. And over the next generation or two, in the aftermath of enlightenment, revolution, and industrialization, European intellectuals everywhere rediscovered the spirit and beauty of an ancient order animated in the main, as they saw it, by Catholic Christianity.[5] Toward the end of the century, in 1879, Pope Leo XIII challenged his Catholic flock to recover their medieval philosophical heritage and to refute modern heresy. *Aeterni patris* laid the foundations for a remarkable neo-Thomist revival that has shaped Catholic thought into the present day.[6] At the turn of the century, the pope's more specifically apologetic purposes combined with a general European flowering of historical and philosophical learning to produce a brilliant revival of medieval scholarship, with Christian philosophy very much at its center. Best known among the institutes that encouraged the new scholarship are those associated with Maurice DeWulf and Fernand van Steenberghen in Louvain, Fernand Grabmann in Munich, Martin Mandonnet in Fribourg, and Etienne Gilson in Toronto. The first volume of *Mediaeval Studies*, official organ of the Pontifical Institute in Toronto, carried a letter from the archbishop of Toronto, its titular head, blessing this enterprise for "applying the best methods of scientific scholarship to the study and interpretation of the thought of those Ages of Faith." A paperback series on philosophy published in the 1950s included a volume on the medieval period called *The Age of Belief*. The editor of that book noted that, "as we become increasingly aware of our own spiritual bankruptcy, we may look

et religion: Recherches sur les utilisations marginales de l'écrit dans la culture populaire du haut moyen âge," in Pierre Boglioni, ed., *La culture populaire au moyen âge* (Quebec, 1979), 123–43; Patrick Geary, "Humiliation of Saints," in Stephen Wilson, ed., *Saints and Their Cults: Studies in Religious Sociology, Folklore, and History* (Cambridge, 1983), 123–40; Edward Peters, *The Magician, the Witch, and the Law* (Philadelphia, 1978); Jeffrey Russell, *Witchcraft in the Middle Ages* (Ithaca, N.Y., 1972); and Raoul Manselli, *Magia e stregonaria nel Medio Evo* (Turin, 1976). For a useful historiographic discussion, see Peters, *The Magician, the Witch, and the Law*, 203–12.

[4] J. van Herwaarden and R. de Keyser, "Het gelovige volk in de late middeleeuwen," in *Algemene Geschiedenis der Nederlanden*, 4 (Haarlem, 1980): 420.

[5] R. Samuel, ed., *Die Christenheit oder Europa*, in Novalis, *Schriften*, 4 vols. (Stuttgart, 1960), 3: 507; and René de Chateaubriand, *Génie du Christianisme*, ed. Pierre Reboul, 2 vols. (Paris, 1966). There is no single synthetic work on "medievalism" in the Romantic era. But see Raoul Manselli, "Il medioevo come *Christianitas*: Una scoperta romantica," in Vittorio Branca, ed., *Concetto, storia, miti e immagini del medioevo* (Venice, 1973), 51–89; Gatto, *Viaggi intorno al concetto*, 107–21; Voss, *Mittelalter im historischen Denken Frankreichs*, 312–38; and Peter Burke, *Popular Culture in Early Modern Europe* (New York, 1978), 3–22. Burke linked the Romantic notion of medievalism to the discovery in the early nineteenth century of "popular" culture and folklore.

[6] See the essays in V. B. Brezik, ed., *One Hundred Years of Thomism* (Houston, 1981). On the context of *Aeterni Patris*, see Pierre Thibault, *Savoir et pouvoir: Philosophie thomiste et politique clericale au XIX^e siècle* (Quebec, 1972).

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back with a certain appreciation—and even with a certain nostalgia—to those spiritually solvent centuries, to that Age of Belief." Through parochial education such views came to permeate the Catholic community.[7] But, outside it as well, scholars and ordinary folks alike looked on medieval civilization as predominantly "Christian" or "Catholic" in character.

Just over a decade ago, Jean Delumeau, one of the first Catholics admitted to the prestigious Collège de France, vigorously contested what he called the "legend of the Christian Middle Ages." Together with Keith Thomas, Carlo Ginzburg, Gerald Strauss, Robert Muchembled, and several others, he argued that medieval folk were at best only superficially Christianized; Christian faith and practice first took hold among the European masses during the Reformation and Counter Reformation movements.[8] These historians of the early modern era were, at least in part, still engaging in the old practice of setting up a "medieval straw man" to be conveniently knocked over by Luther, Calvin, and Trent. But this interpretation rapidly gained adherents among historians of the medieval period as well. Gabriel Le Bras questioned fashionable new arguments about the origins and pace of Europe's "de-Christianization" on the grounds that medieval Europe was in reality never all that thoroughly Christianized. André Vauchez and Paolo Brezzi, both Catholic, likewise repudiated the "myth of the Christian Middle Ages."[9] Even in studies of medieval philosophy, which were once central to the notion of a Christian medieval Europe, images of a prevailing "Christian spirit" have yielded to an emphasis on logic and nonmetaphysical issues and an insistence on many differing schools of thought. Indeed, one scholar (also a Catholic priest) has interpreted Dante as secretly skeptical of Christian claims to philosophical truth.[10]

[7] See the page facing the imprimatur in the first volume of *Mediaeval Studies* (1939). On Toronto's institute, see Lawrence K. Shook, *Catholic Post-Secondary Education in English-Speaking Canada: A History* (Toronto, 1971), 210–28, and *Etienne Gilson* (Toronto, 1984), 180, 192–95; Anne Fremantle, *The Age of Belief* (New York, 1954), x; and Philip Gleason, "Mass and Maypole Revisited: American Catholics and the Middle Ages," *Catholic Historical Review*, 57 (1971): 249–74.

[8] Delumeau, *Le catholicisme entre Luther et Voltaire*, Nouvelle Clio, no. 30 (Paris, 1971), 234–35, and "Au sujet de la déchristianisation," *Revue d'histoire moderne et contemporaine*, 22 (1975): 57. On Delumeau and his views, see M. Despland, "How Close Are We to Having a Full History of Christianity? The Work of Jean Delumeau," *Religious Studies Review*, 9 (1983): 24–33. Also see Muchembled, *Culture populaire et culture des élites dans la France moderne (XV^e–XVIII^e siècles)* (Paris, 1978), 131–33; Ginzburg, *The Cheese and the Worms* (New York, 1982), esp. 126; and Strauss, *Luther's House of Learning: Indoctrination of the Young in the German Reformation* (Baltimore, Md., 1978), 268–308. For a good review of this literature and viewpoint, see Natalie Z. Davis, "From 'Popular Religion' to Religious Cultures," in Steven Ozment, ed., *Reformation Europe: A Guide to Research* (St. Louis, Mo., 1982), 321–42. For another approach, see Jean Wirth, *Luther: Etude d'histoire religieuse* (Geneva, 1981), 35–90.

[9] Le Bras, "'Déchristianisation': Mot fallacieux," *Cahiers d'histoire*, 9 (1964): 92–97; and Delumeau, "Au sujet de la déchristianisation," 52–60. Also see Brezzi, *Metodologia storiografica e problematica medioevale* (Rome, 1975), esp. 163–94; and Vauchez, "Les nouvelles orientations de l'histoire religieuse de la France médiévale (avant le XIII^e siècle)," in his *Religion et société dans l'occident médiévale* (Turin, 1980), 293: "La renaissance catholique de la première moitié du XIX^e siècle qui se fondait sur une vision mythique de la Chrétienité."

[10] See Etienne Gilson, *The Spirit of Mediaeval Philosophy* (New York, 1936), 404. Gilson in summary of his book described himself as "wishing to set forth a spirit of medieval philosophy, and [as] having identified it with the spirit of Christian philosophy. Compare the introduction to Norman Kretzmann *et al.*, eds., *The Cambridge History of Later Medieval Philosophy*, (Cambridge, 1982), 3. The *Cambridge History*, with its quite different emphases, was written partly as a pedagogical corrective, but it was also meant, in the editors' words, to "help end the era during which [medieval philosophical thought] has been studied in a philosophical ghetto." Jaroslav Pelikan made similar remarks. See Pelikan, "The

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The old Romantic image, in short, has been turned on its head. Jacques Le Goff concluded a lengthy survey of medieval Christianity by declaring that "towards 1500 Europe was still virtually mission country."[11]

Were the Middle Ages then a flourishing epoch of Catholic Christianity or a millennium of Indo-European folklore? A number of studies, published since the early 1970s, have offered an easy, but inadequate, answer. The traditional achievements of the Christian Middle Ages are ascribed to an educated "clerical elite" and the folkloric practices pervasive in the medieval era, and newly appreciated by historians, to the rest of the "people." More noteworthy than the solution, it seems to me, was the emergence of the issue. That such a question—whether medieval culture was essentially "Christian" and what that might mean—could become central to medieval historiography reflected a crucial change. Until fifty years ago the study of medieval religious life was left largely to theologians, church historians, and members of religious orders. Richard Southern recently made this point in the wry opening sentence of his appreciation of Beryl Smalley, his lifelong colleague. "In 1927, when Beryl Smalley began to study the Bible in the Middle Ages," he wrote, "I think it would be true to say that the Bible had almost no place in the minds of medieval historians."[12] The present debate derives in no small measure from the emergence of medieval religious life as a field of historical inquiry fully as legitimate as medieval politics, warfare, and economics. How that happened, and its impact on our view of the Middle Ages, can most easily be understood by examining the works and perspectives of influential scholars and by outlining three general interpretive tendencies.[13] A brief word must also be said about the continuing—and yet changing—influence of confessional considerations, that is, perspectives influenced by or derived from personal religious convictions or affiliations.

IN HIS DISSERTATION, COMPLETED IN 1927, Herbert Grundmann (1902–70) argued that Joachim of Fiore was neither a dangerous "heretic" nor a wild-eyed "millenarian" but a significant witness to and product of the intellectual and

Middle Ages as 'Age of Faith,'" in his *The Growth of Medieval Theology* (Chicago, 1978), 1–8. Also see Ernest L. Fortin, *Dissidence et philosophie au moyen âge: Dante et ses antécédents* (Montreal, 1983), esp. 160.

[11] Le Goff, "Le christianisme médiévale en occident du concile de Nice (325) à la réforme (début du XVI[e] siècle)," in *Histoire des religions*, Encyclopédie de la Pléiade (Paris, 1972), vol. 2, p. 856: "La Chrétienté vers 1500 c'est, presque, un pays de mission."

[12] Southern, "Beryl Smalley and the Place of the Bible in Medieval Studies, 1927–1984," in Katherine Walsh and Diana Woods, eds., *The Bible in the Medieval World: Essays in Memory of Beryl Smalley* (Oxford, 1985), 1.

[13] A full survey of recent scholarship in medieval religious history is not possible here. For general orientations, see François Rapp, "Réflexions sur la religion populaire au moyen âge," in Bernard Plongeron, ed., *La religion populaire: Approches historiques* (Paris, 1976), 51–98; Bernard McGinn, "Medieval Christianity: An Introduction to the Literature, 1957–1977," *Anglican Theological Review*, 60 (1978): 278–305; Le Goff, "Le christianisme médiévale"; and Vauchez, "Nouvelles orientations de l'histoire religieuse." On the larger interpretative issues, see the essays in *Cristianesimo nella storia*, 5 (1984): 29–166.

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spiritual milieu of his time.[14] To get at that milieu, that larger setting, required another decade of work before Grundmann published his monumental *Religious Movements in the Middle Ages*, which has become the foundation for the historical study of medieval religious life.[15] Most of its themes are now commonplace: the twelfth-century quest for the "apostolic life," with particular sensitivity to the issue of poverty; the common inspiration of groups ending up both inside and outside the church (with comparisons of Francis and Waldo); the centrality of women in those movements; Innocent III's skillful and crucial handling of the mendicants; and the rise of a vernacular religious culture for the laity, especially lay women. Yet, just because so much subsequent work still stands in the shadow of this book, Grundmann's distinctly personal approach deserves closer analysis.

The focal point, both scholarly and personal, for all his work remained the individual quest for the truly apostolic (that is, Christian) life.[16] Grundmann's interest and exposition normally stopped short the moment any movement gained official approbation as an order or reprobation as a sect—the only two long-term options, in his view, possible within the structures of the medieval church. In general, Grundmann was more taken therefore with hermits and wandering preachers than with professed monks, with adult converts than with child oblates, with religious women still partly in the world (Beguines) than with nuns, and with individuals making their own spiritual way (Waldo, Francis) than with spokesmen for established groups.[17] Individualistic "heresiarchs" deserved sympathetic study; "heretics" or the "religious" who joined groups for familial, social, or cultural reasons he mostly left to others. As the new orders produced by religious movements grew ever more "monkish," he saw that fringe groups would inevitably turn all the more "radical."[18]

What Grundmann taught historians above all was to take seriously both the centrality of these movements in medieval history and the religious motivation of

[14] Grundmann, *Studien über Joachim von Fiore* (1927; reprint edn., Darmstadt, 1966). It is striking how many scholars sought a new and less confessional view of medieval religious life by way of studying Joachim. See Emile Gebhart, *Introduction à l'histoire du sentiment religieux en Italie* (Paris, 1884), and *L'Italie mystique* (3d edn., Paris, 1899); Paul Fournier, *Etudes sur Joachim de Fiore et ses doctrines* (Paris, 1899); Marjorie Reeves, *The Influence of Prophecy in the Later Middle Ages* (Oxford, 1969); Norman Cohn, *The Pursuit of the Millennium* (London, 1957); Ernst Benz, *Ecclesia Spiritualis* (Stuttgart, 1934); Grundmann, *Studien über Joachim von Fiore*; Giovanni Volpe, *Movimenti religiosi e sette ereticali nella società medievale italiana* (Florence, 1922); Ernesto Buonaiuti, *Gioacchino da Fiore: I tempi, la vita, il messaggio* (Rome, 1931); and Raffaelo Morghen, *Medioevo cristiano* (Rome, 1951). Reeves began her work in the 1930s. One may wonder in retrospect whether studies of Joachim offered the best approach to medieval religious culture.

[15] Grundmann, *Religiöse Bewegungen im Mittelalter* (1935; reprint edn., Darmstadt, 1970). For a good introduction to Grundmann's work, see the necrology by Arno Borst, in Grundmann, *Ausgewählte Aufsätze*, Monumenta Germaniae Historica, Schriften, no. 25 (Stuttgart, 1976–78), 1: 1–25. The volume includes a complete bibliography. See Grundmann, *Ausgewählte Aufsätze*, 1: 26–37. Also see R. Büchner, "Religiösität, Spiritualismus, geistige Armut, Bildung: Herbert Grundmann's geistesgeschichtliche Studien," *Innsbrucker Historische Studien*, 1 (1978): 239–51.

[16] See Borst's comment in Grundmann, *Ausgewählte Aufsätze*, 1: 2: "Ein eigenes Verständnis vom christlichen Evangelium und seiner Verwirklichung; hier liegt die tiefste Würzel für sein Lebenswerk."

[17] Grundmann, *Religiöse Bewegungen*, 5–6. All these themes, inherent in *Religiöse Bewegungen*, were developed in separate essays under that same title and gathered into volume 1 of his *Aufsätze*.

[18] See, especially, Grundmann, "Hérésies savantes et hérésies populaires au moyen âge," in *Ausgewählte Aufätsze*, 1: 417–22, and his "Neue Beiträge zur Geschichte der religiösen Bewegungen im Mittelalter," in *Religiöse Bewegungen*, 513, and *Ausgewählte Aufsätze*, 1: 67.

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their adherents regardless of origins, be they Cathars or Free Spirits.[19] But, while he was clear that these religious movements should not be dismissed by church historians as "heretical" or interpreted by general historians as "social protest," Grundmann never defined exactly what he understood by the term "movement." His own book was really a series of connected essays, and he virtually ignored routine religious practices and institutional developments. (Interestingly, Grundmann himself ceased formal practice in the State Lutheran Church.) Indeed, the institutionalization of a religious movement seemed to him neither desirable nor possible, so—to redo his title—ordinary religious life (or practice) in the Middle Ages remained largely untreated and, by implication, unworthy of treatment, since those "moved" to "religion" had to go beyond it.

By contrast, for Etienne Delaruelle (1904–71), a French Catholic, the heart of medieval religious life lay in the "piety" of the "people."[20] Like Grundmann, he was committed to the exposition of authentic Christian experience and much less interested in socioeconomic questions. But he searched for that Christianity in collective expressions of piety, such as crusades and pilgrimages, and in common cults, like those of the cross and the saints. He knew well enough that popular religiosity often expended itself in practices verging on magic, varieties of gross superstition, and perfunctory external rites, but he intuitively focused elsewhere, on taking up the cross to go on crusade rather than on its attendant indulgences, on pilgrimages as penitential acts rather than on shrines, on devotion to the saints rather than on miracles and magic. He also believed that medieval religious life underwent an increasing interiorization—bringing a deepened personal appropriation, moving away from crusades and pilgrimages, for instance, and toward observances such as the stations of the cross. Delaruelle steadily resisted the identification of medieval religious life with either the "professionally" or the "extraordinarily" (that is, heretically) religious, but he conceded deep "tension, even opposition," between clerical and popular Christianity, and he saw Saint Francis as the only figure truly to transcend it.[21] Nonetheless, Delaruelle's work, exemplified best in his contribution to the fourteenth volume in the series Histoire de L'église, effectively brought the piety of ordinary people into the mainstream of historical study (and, in another context, directly influenced Vatican Council II). Yet Delaruelle never defined these "people" conceptually or socially, which was

[19] For works by two excellent disciples of Grundmann, see Arno Borst, *Die Katharer*, Monumenta Germaniae Historica, Schriften, no. 12 (Stuttgart, 1953); and Robert E. Lerner, *The Heresy of the Free Spirit* (Princeton, N.J., 1972).

[20] Delaruelle never published his thesis on Catherine of Siena. For his most important works, see *L'église au temps du Grand Schisme et de la crise conciliare*, Histoire de l'église, no. 14 (Paris, 1964), *La piété populaire au moyen âge*, ed. R. Manselli and A. Vauchez (Turin, 1975), and *L'idée de croisade au moyen âge*, ed. R. Richard (Turin, 1980). Both collections of essays have important introductions to Delaruelle and his work. Also see M. H. Vicaire, "L'apport d'Etienne Delaruelle aux études de spiritualité populaire médiévale," in *La religion populaire en Languedoc*, Cahiers de Fanjeaux, no. 11 (Toulouse, 1976), 23–36. Vicaire offered a positive evaluation of Delaruelle's work. For a more negative view, see J. C. Schmitt,"'Religion populaire' et culture folklorique," *Annales, économies, sociétés, civilisation*, 31 (1976): 941–53.

[21] For the closest Delaruelle came to a definition of "popular piety," see his "La vie commune des clercs et la spiritualité populaire au moyen âge," and "Saint François d'Assise et la piété populaire," in *La piété populaire au moyen âge*, 165–67, 275.

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one of Jean-Claude Schmitt's main criticisms, nor did he confront squarely the gap in piety between their actual practice and the formal prescriptions of the church.

Jean Leclercq (1911–), Delaruelle's contemporary, introduced historians to monastic piety in such a way—and so successfully—that numerous lay and even agnostic scholars are focusing sympathetically on monastic spiritual life, an undertaking almost inconceivable two generations ago. Building on the work of André Wilmart and others, Leclercq placed the development of the monastic orders within general cultural and literary history and transformed monastic "spirituality" into a coherent world view or outlook, which enabled historians to fit monastic thought and devotional literature far better into the larger medieval story.[22] But Leclercq's "monastic theology" lacks firm historical definition and is therefore subject, like Grundmann's "religious movements" and Delaruelle's "popular piety," to a certain vagueness or even opportunism in its concrete historical applications. At the same time, and in much the same context, Marie-Dominique Chenu and Yves Congar attempted to elucidate theological and ecclesiological themes contextually.[23] Their purpose, much like that of Delaruelle and Leclercq, was to reform and to promote from within the church through historical understanding, but their effect—not altogether unintended—was to make these materials generally accessible and thus to draw many others into their study.

If Grundmann established the study of medieval "religious movements," Delaruelle "popular piety," and Leclercq "monastic theology," Gabriel Le Bras (1891–1970) founded "religious sociology." An expert in history and law, Le Bras approached religion from the perspective of its organization and practice, in an effort to explain historically the variety and degree of Catholic practice observable in modern France.[24] In terms of the *Annales* school, what Delaruelle did for religious "mentalities," Le Bras undertook, at Marc Bloch's specific encouragement, for religious "societies." He called for a "religious geography" to map out local practice and the physical state of churches and cemeteries, studies of parishes

[22] Leclercq's most influential work was *The Love of Learning and Desire for God* (2d paperback edn., New York, 1974), first published in 1957 under the title *L'amour des lettres et le désir de Dieu: Initiation aux auteurs monastiques du moyen âge*. For a complete bibliography, see Réginald Grégoire, "Bibliographie de Dom Jean Leclercq," *Studia Monastica*, 10 (1968): 161–80, and 20 (1978): 409–23. For a critical review of Leclercq's historical contribution, see G. G. Meerseman, "'Teologia monastica' e riforma ecclesiastica da Leone IX a Callisto II," in *Il monachesimo e la riforma ecclesiastica, 1049–1123* (Milan, 1971), 257–70; and Gregorio Penco, "La teologia monastica: Bilancio di un dibattito," *Benedictina*, 26 (1979): 189–98. For a partial redefinition of monastic theology, see Leclercq, "The Renewal of Theology," in Robert Benson *et al.*, eds., *Renaissance and Renewal in the Twelfth Century* (Cambridge, 1982), 68–87, esp. 71 n. 7.

[23] Chenu, *La théologie au douzième siècle* (Paris, 1957), and *Introduction à l'étude de Saint Thomas d'Acquin* (Paris, 1950); Congar, *L'ecclésiologie du haut moyen âge* (Paris, 1968), and *L'église de Saint Augustin à l'époque moderne* (Paris, 1970). Chenu's *La théologie au douzième siècle* has been partially translated by Lester K. Little and Jerome Taylor as *Nature, Man, and Society in the Twelfth Century* (Chicago, 1968). Also see the essays by Congar in *Droit ancien et structures ecclésiales* (London, 1982).

[24] The best approach to Le Bras is through his two-volume *Etudes de sociologie religieuse* (Paris, 1955–56), where his most important essays have been organized, as it were, programmatically. For a complete bibliography, see *Etudes d'histoire du droit canonique dediés à Gabriel Le Bras*, 2 vols. (Paris 1965), 1: ix–xxxiii. And for a useful interpretative introduction, see Henri Desroche, "Areas and Methods of a Sociology of Religion: The Work of G. Le Bras," *Journal of Religion*, 35 (1955): 34–47. The subtitle of the first essay (from 1931) in his *Etudes d'histoire* reads "Pour un examen détaillé et pour une explication historique de l'état du Catholicisme dans les diverses régions de la France."

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and confraternities to get at local social organization and of canon law, visitation records, synodical statutes, and parish registers to get at local sources. His own masterful account of medieval ecclesiastical institutions revealed a Latin Christendom comprised of several overlapping and competing interest groups (cardinals versus popes, chapters versus bishops, religious versus seculars, parishoners versus vicars, and so on)—all greatly differentiated in time and place and yet, somehow, in the High Middle Ages, roughly amalgamated into a "common law" of the church, a term that suggests "practice" becoming "norm." Such close examination permanently shattered any hazy Romantic notions of medieval Christendom as singularly unified or pious and disclosed enormous gaps between ideal and reality, Christian doctrine and popular practice.[25] Yet Le Bras's own inspiration clearly arose out of a scholarly effort to recover his childhood experience in a village parish. A posthumous book on "the church and the village" described each village church as a "religious capital" and looked back nostalgically on a time when village life, in all its "lived" dimensions, revolved around the church.[26]

In England, meanwhile, where the adage "history is past politics" reigned into the 1920s, a historian of strict Dissenter stock established the study of medieval religious life. Maurice Powicke (1879–1963), together with David Knowles (1896–1974) of Benedictine and neo-Scholastic background, produced in Richard Southern, Beryl Smalley, Christopher Cheney, Christopher Brooke, and many others a generation or two of rare accomplishment. Powicke, notably for a Dissenter, concentrated attention on medieval Christianity's mainstream: the prescriptions of councils and synods, the teachings of schoolmen, the lives of monks. Yet he also said of medieval religious life: "We no longer believe in that well-behaved body of the faithful. . . . Paganism abounded, but it was the literal paganism of the natural man."[27] Powicke's perception of a blend between the "naturally pagan" and mainstream Christianity's finest achievements was to find a continuing echo. Richard Southern (1911–) described medieval civilization as a mix between "the practical, business-like and earthy, and the intellectual, spiritual

[25] This more critical vision of medieval Christianity is particularly evident in two dissertations directed by Le Bras. See Paul Adam, *La vie paroissiale en France au XIV^e siècle* (Paris, 1964); and Jacques Toussaert, *Le sentiment religieux en Flandre à la fin du moyen âge* (Paris, 1963). Toussaert's book, widely used in France and America, received critical reviews in Flanders. For a general appraisal with references to other reviews, see Ludo Milis, "De devotionele praktijk in de laat-middeleeuwse Nederlanden," in J. D. Janssens, ed., *Hoofsheid en devotie in de middeleeuwse maatschappij* (Brussels, 1982) 133–45, and 144 n. 10.

[26] See Le Bras, *Prologomènes*, Histoire du droit et des institutions de l'église en Occident, no. 1 (Paris, 1955), and *Institutions ecclésiastique de la chrétienité médiévale*, Histoire de l'église, no. 12 (1959/1964). Compare his *L'église et le village* (Paris, 1976). Le Bras's work was the inspiration for the "new" history of "lived" Christianity. See Jean Delumeau, ed., *Histoire vécue du peuple chrétien*, 2 vols. (Toulouse, 1979). In general Le Bras's approach has proved much more fruitful for the study of early modern than of medieval Christianity because of the far greater supply of documentary sources available after about 1400.

[27] Powicke, *Christian Life in the Middle Ages and Other Essays* (Oxford, 1935) 5, 3. For an excellent appreciation of Powicke with bibliography by Richard W. Southern, see *Proceedings of the British Academy*, 50 (1964): 275–304. For an earlier bibliography, see *Studies in Medieval History Presented to Frederick Maurice Powicke* (Oxford, 1948), 469–91. For further insight into the historiographical shift since the 1930s, see Richard Southern, "Marjorie Reeves as an Historian," in Ann Williams, ed., *Prophecy and Millenarianism: Essays in Honour of Marjorie Reeves* (Burnt Hill, England, 1980), 3–9.

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and aspiring." His survey of the medieval church, perhaps the most influential of the last generation, dealt primarily with the practical, "the connexion between religious organizations and . . . social setting," while leaving the intellectual and spiritual to another volume on Scholasticism, which is still awaited. Partly, like Leclercq, under Wilmart's influence, Southern identified the "aspiring" especially with monastic spirituality, a theme most evident in his works on Anselm.[28] Smalley (1906–84) focused on the schoolmen's literal and historical study of the Bible, though one of her best books took up precisely the same juxtaposition—intellectuals in politics—that Southern highlighted.[29] This notion of a "mix" between the natural and the spiritual man, though much milder in its rhetoric, proved as corrosive of the old Romantic images of medieval Christianity as the ideas of Delumeau, Le Bras, and their colleagues. It was Christopher Cheney (1905–), after all, who laid out the realities of ecclesiastical administration,[30] Smalley who demonstrated the Jewish community's role in developing Christian exegesis, and Southern who discarded as myths the schools of Chartres and early Oxford and insisted that universities were meant to serve very practical ends.[31]

While these scholars, active in England, Germany, and France since the 1920s and 1930s, laid the foundation for a study of medieval religious life in both its ideals and its practical realities, another group, mostly younger and influenced in part by church reform and countercultural movements, set out to explore and to include the religious aspirations of medieval fringe groups. This was a general phenomenon, associated, for instance, with Norman Cohn's book *The Pursuit of the Millennium* (1957),[32] but the approach took especially pronounced forms among scholars in countries with crumbling Catholic orthodoxies and is nowhere better illustrated than in the work of two Italian historians, Raffaello Morghen (1895–1983) and Raoul Manselli (d. 1984). Morghen insisted that "the fundamental theme inspiring and permeating the whole development of medieval

[28] Southern, *Medieval Humanism and Other Studies* (New York, 1970), vii, and *Western Society and the Church in the Middle Ages* (Baltimore, Md., 1970), 15. The essay "Medieval Humanism" in the first book represents, in effect, the paradigmatic interpretation of the greatest achievement of the High Middle Ages as a successful synthesis between the "natural" and the "spiritual." For a bibliography of Southern's works, see R. H. C. Davis and J. M. Wallace-Hadrill, eds., *The Writing of History in the Middle Ages: Essays Presented to Richard William Southern* (Oxford, 1981), 495–502.

[29] For insights into Smalley's and Southern's historiographical positions, see Southern, "Beryl Smalley and the Place of the Bible in Medieval Studies," 1–16. Also see Smalley's preface to the third edition of *The Study of the Bible in the Middle Ages* (Oxford, 1983), vii–xvii. The title of Smalley's *The Becket Conflict and the Schools: A Study of Intellectuals in Politics* (Totowa, N.J., 1973) is noteworthy. For a collection of her essays and a bibliography of her work, see *Studies in Medieval Thought and Learning from Abelard to Wyclif* (London, 1981), esp. 417–22.

[30] Cheney's work is well represented in his collected studies, *Medieval Texts and Studies* (Oxford, 1973), and in *From Becket to Langton: English Church Government, 1170–1213* (Manchester, 1956) and *Pope Innocent III and England*, Päpste und Papsttum, no. 9 (Stuttgart, 1976). For a complete bibliography to 1975, see *Church and Government in the Middle Ages: Essays Presented to C. R. Cheney on his 70th Birthday* (Cambridge, 1976), 275–84. The juxtaposition evident in the title *Church and Government* is again noteworthy.

[31] Southern, "The Schools of Paris and the School of Chartres," in Benson *et al., Renaissance and Renewal*, 113–72, and "From Schools to University," in J. I. Catto, *The History of the University of Oxford* (Oxford, 1984), vol. 1, pp. 1–36.

[32] See Jeffrey Russell and Carl Berkhout, *Medieval Heresies: A Bibliography, 1960–79* (Toronto, 1981). For a review of Russell's *Lucifer: The Devil in the Middle Ages* (Ithaca, N.Y., 1984), see Norman Cohn, "Giving the Devil His Due," *New York Review of Books*, April 25, 1985, pp. 13–14.

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civilization was the Christian religious tradition." But he saw its force as divided between a clerical hierarchy and an unchanneled, virtually uncontrollable religious movement rooted in the "religious conscience of lay society, which rose together with the renewed life of the city and the communes." Joachim, he contended, not Pope Innocent or Friar Thomas, was "the greatest exponent, master, and prophet of all the spirituality of the thirteenth century."[33] So, too, Manselli, Morghen's disciple, specifically defended the Romantic notion of a medieval Christian civilization but located it in "foundational" values rather than hierarchical structures and interpreted critical and heretical movements as signs of vitality. Accordingly, the focus of his research was—to paraphrase the titles of his major works—heretical reformers, Franciscan spirituals, religious deviants, popular religiosity, magic and witchcraft, and, lastly, Saint Francis.[34] His entire approach wholly presupposed an institutional church, yet he tended to put the "truly religious" outside it. Manselli sought to include "popular" and even "fringe" elements in the medieval church but was often unclear about their relationship to each other and to medieval Christianity as a whole, and, while rejecting both neo-Scholastic and social determinist interpretations, he still attempted to understand medieval Christianity as a socioreligious unity.

All the historians described thus far, whatever their emphasis or approach, had, it is fair to say, a personal religious interest in the study of a Christian medieval civilization. A new school has emerged comprised of historians, represented here by Jacques Le Goff and Jean-Claude Schmitt, who have approached medieval religious life as historical anthropologists investigating a native culture subjected to the propaganda of Christian missionaries. Missionary is, roughly, Schmitt's image, in his book *The Holy Greyhound*,[35] of Stephen of Bourbon, the Dominican inquisitor. Le Goff first propagated the notion of a wholly distinct religious culture among the people, worthy of serious study and essential to an understanding of earlier European life. From the beginning, he has had a flair for dramatic pronouncements, choosing to write a textbook rather than a *thèse d'état*, to collect his essays under the programmatic French title *Pour un autre moyen âge*, and to focus his one major historical monograph on purgatory, understood as a creative new

[33] Morghen, *Medioevo Cristiano* (1951; 4th edn., Rome, 1974), 19, 253, 257. For a bibliography of Morghen's works, see *Studi sul medioevo cristiano offerti a R. Morghen*, 2 vols. (Rome, 1974), 1: xi–xxi. Also see Morghen, "La lezione di Medioevo Cristiano," *Quaderni catanesi*, 2 (1980): 453–68, and "Cultura laica e cultura cattolica in Roma ai primi del'900," *Archivio della società romana di storia patria*, 102 (1979): 393–411. By way of his teacher, Ernesto Buonaiuti, Morghen had intellectual contacts with Modernist historians condemned by the hierarchy in the early twentieth century. Among Morghen's own students, Manselli studied "heretics" and Arsenio Frugoni examined a radical evangelical critic of the Roman papacy in the twelfth century. See Frugoni, *Arnoldo da Brescia* (Rome, 1954). On Frugoni and his collected essays, see Giovanni Miccoli, "Gli *Incontri nel Medio Evo* di Arsenio Frugoni," *Studi Medievali*, 24 (1983): 469–86.

[34] Manselli, "La Christianitas medioevale di fronte all'eresia," in Branca, *Concetto, storia, miti e immagini*, 90–133, esp. 132–33, *Studi sulle eresie del secolo XII* (Rome, 1953), *Spirituali e Beghini in Provenza* (Rome, 1959), *I fenomeni di devianza nel medioevo* (Turin, 1972), *Magia, and San Francesco d'Assisi* (Rome, 1981). Manselli's personal views are most evident in the opening and closing chapters of his *La religion populaire au moyen âge* (Montreal, 1975). For a bibliography of his relevant work, see *La religion populaire*, 14–15.

[35] Schmitt, *The Holy Greyhound: Guinefort, Healer of Children since the Thirteenth Century* (Cambridge, 1983), 11–35.

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product of medieval man's spatial imagination.[36] With this school, and with numerous historians adopting related approaches, the study of medieval religious life has in a sense come full circle. Practices that earlier generations of Protestants and enlightened liberals dismissed as so much superstition, and many Catholics quietly downplayed or entirely overlooked, have been taken up enthusiastically—with a heavy debt to anthropologists and folklorists—as the authentic (and non-Christian) religion of the people.[37]

The study of both comparative religions and folklore dates from the nineteenth century, and both disciplines have been fitfully applied to the study of medieval religious life. There was a fierce debate in the early twentieth century over the cult of the saints and their relics as a "pagan survival,"[38] and several German scholars, especially Catholics, attempted in the 1930s to elucidate sympathetically the role of folklore in various aspects of medieval religious life.[39] Le Goff, Schmitt, and others have now set out to draw far more systematically on structural analysis as a way of getting at a submerged popular culture in which religion—that is, a religion common to Indo-European peoples—played a key role as a cohesive force. At the heart of Le Goff's and Schmitt's vision is an argument for two distinct cultures, the one clerical and bookish, the other popular, oral, and customary, the first accessible through traditional intellectual and spiritual categories, the second mainly through cultural anthropology and comparative religions.[40] Further, these historians have argued that bookish, clerical culture was to some degree an

[36] Le Goff's *Pour un autre moyen âge* (Paris, 1977) has been translated. See Le Goff, *Time, Work, and Culture in the Middle Ages*, trans. Arthur Goldhammer (Chicago, 1980). For an illuminating preface on Le Goff's outlook, see *Time, Work, and Culture*, vii–xv. Also see Le Goff, *The Birth of Purgatory*, trans. Arthur Goldhammer (Chicago, 1984). For reviews of Le Goff's works, see *Times Literary Supplement*, June 18, 1982, pp. 651–52; Adriaan Bredero, "Le moyen âge et le purgatoire," *Revue d'histoire écclésiastique*, 78 (1983): 429–51; Aaron Gurevich, "Popular and Scholarly Medieval Cultural Traditions: Notes in the Margins of Jacques Le Goff's Book," *Journal of Medieval History*, 9 (1983): 78–98; and E. Mégier, "Deux examples de 'prépurgatoire' chez les historiens: A propos de *La naissance du Purgatoire* de Jacques Le Goff," *Cahiers de civilisation médiévale*, 28 (1985): 45–62. There is a generally positive presentation of this whole "French" school (but with examples mostly from modern history) by Willem Frijhoff; "Van 'Histoire de l'église' naar 'histoire religieuse,'" *Nederlands Archief voor Kerkgeschiedenis*, 61 (1981): 113–53.

[37] For the clearest presentation of Le Goff's thesis, see his "Clerical Culture and Folklore Traditions in Merovingian Civilization" and "Ecclesiastical Culture and Folklore in the Middle Ages: Saint Marcellus of Paris and the Dragon," in *Time, Work, and Culture*, 153–88. Also see Schmitt, "'Religion populaire' et culture folklorique," and "'Jeunes' et danse des chevaux de bois: Le folklore méridional dans la litterature des *exempla* (XIII^{e}–XIV^{e} siècles)," in *Religion populaire en Languedoc*, 127–58.

[38] For the best and most balanced survey of a large body of works, see František Graus, *Volk, Herrscher und Heiliger im Reich der Merowinger: Studien zur Hagiographie der Merowingerzeit* (Prague, 1965).

[39] German scholars had a tradition of work in "Volkskunde" going back at least to the Brothers Grimm. See Heinrich Schauerte, "Entwicklung und gegenwärtiger Stand der religiösen Volkskundeforschung," *Historisches Jahrbuch*, 72 (1953): 516–34. Peter Browe, Otto Höfler, and especially Georg Schreiber (1882–1963) were among the most significant of these scholars. On Schreiber, see Nikolaus Grass, "In Memoriam," *Zeitschrift der Savigny-Stiftung für Rechtsgeschichte, Kanonistische Abteilung*, 49 (1963): 621–26. Broadly speaking, German scholarship in "religious folklore" declined after the Second World War, and French work, stimulated by structural anthropology, increased.

[40] For the most explicit expression of structuralism, see Le Goff, "Melusina: Mother and Pioneer," in *Time, Work, and Culture*, 205–22; and Schmitt, *The Holy Greyhound*, 40–48. Also see, for Claude Brémond's structural analysis of an exemplum, Jean-Claude Schmitt *et al.*, *L'exemplum*, Typologie des sources, no. 40 (Turnhout, 1981), 113. This interpretive approach also underlies R. Howard Bloch's *Etymologies and Genealogies* (Chicago, 1983). For far greater skepticism about the reliability of literary accounts of "superstition," see Dieter Harmening, *Superstitio: Überlieferungs- und theoriegeschichtliche Untersuchungen zur kirchlich-theologischen Aberglaubensliteratur des Mittelalters* (Berlin, 1979).

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"ideological reflection" of the social elite but operated far more as an instrument of its oppression. This social component, though important to their interpretation, seems the least developed and least convincing. Surely medieval churchmen used their spiritual authority and intellectual abilities to economic and political advantage, as demonstrated, for instance, in Joseph Lynch's study of simoniac entrance into monastic life. But learned clerics comprised, as Le Bras insisted, a congerie of mutually competing interests, not a uniform class, and in any case two-thirds or more of the landed estates—and the power that came with them—were not in the possession of a clerical elite.[41] More important and much more fruitful are the attempts made by this school to dredge up from the bottom, as it were, the residues of peasant religious "folklore." Insights from the analysis of comparative religions have stimulated historians to deal with folk practices on their own terms, and not just as some degraded form of Christianity or as the "paganism of the natural man." These insights have also forced historians to reflect again on the import of the church doing its business and developing its culture in a language unknown to most of its people—a point to which the members of this school repeatedly return.[42] Indeed, their work reflects a broader concern to recover medieval traditions outside those recorded and upheld by a clerical elite.[43]

But to argue that the people had a wholly distinct religious culture, not somehow amalgamated into Christian practice, is quite another matter. Before adopting such a view, historians must examine more closely the sources used to argue for it and the interpretive presuppositions that have guided it. The main evidence cited by proponents of separate cultures derives from *exempla*[44] and from inquisitorial records.[45] Both record the stories and sometimes the words of people

[41] The thesis is generally argued more forcefully by Schmitt; *The Holy Greyhound*, 169–70, and *L'exemplum*, 107: "L'*exemplum* a compte parmi les moyens le plus remarquables des échanges culturels, parce qu'il était d'abord l'un des instruments les plus efficaces du pouvoir idéologique." To view the "clerical establishment" as a relatively unified whole is central to the structure of this thesis. See Le Goff's introduction to Jean-Claude Schmitt, *Mort d'une hérésie: L'église et les clercs face aux béguines et aux béghards du Rhin supérieur du XIV^e^ au XV^e^ siècle* (Paris, 1978), 6. But also see Le Bras, *Institutions écclésiastique*; and Lynch, *Simonical Entry into Religious Life from 1000 to 1250* (Columbus, Ohio, 1976), 25–60. Lynch thought "oppression" stemmed mostly from the perceived and desired religious power of the monastic habit.

[42] See Thomas Tentler, "Seventeen Authors in Search of Two Religious Cultures," *Catholic Historical Review*, 71 (1985): 248–57. Tentler's review here of *Faire Croire: Modalités de la diffusion et de la réception des messages religieux du XII^e^ au XV^e^ siècle*, Collection de l'école française de Rome, no. 51 (Rome, 1981), offers a good schematic survey of the issues. In the introduction to that volume, André Vauchez's "Présentation" (pages 7–16) suggests, in a kind of reversal, that the new learning of a university-trained clergy after 1200 was responsible for dividing the religious culture.

[43] This is explicit throughout Le Goff's *Time, Work, and Culture*. Also see, for instance, Georges Duby's *Medieval Marriage: Two Models from Twelfth-Century France* (Baltimore, Md., 1978). Duby's book contains a similar thesis of two competing cultures, primarily on the social rather than on the religious plane.

[44] *Exempla* were the essential sources for Schmitt's *The Holy Greyhound*. See the joint introduction to this genre by Schmitt, Le Goff, and Bremond in *L'exemplum*. For a good orientation to the sources, see Jean-Claude Schmitt, "Recueils franciscans d'*exempla* et perfectionnement des techniques intellectuelles du XIII^e^ au XV^e^ siècles," *Bibliothèque de l'école des chartes*, 135 (1977): 5–21. For references to more works, mostly on the literary aspects of the *exempla*, see Peter von Moos, "The Use of *exempla* in the *Policraticus* of John of Salisbury," in M. Wilks, ed. *The World of John of Salisbury* (Oxford, 1984), 210–13.

[45] Inquisitorial records were the essential sources for Carlo Ginzburg's *The Cheese and the Worms* and Emmanuel Le Roy Ladurie's *Montaillou, village occitan de 1294 à 1324* (Paris, 1975). On Ladurie's use

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at odds with approved practice or orthodox doctrine. Because these materials originated or first abounded in the thirteenth century, Le Goff and Schmitt have renamed this "age of faith" a great "age of folklore."[46] Most of these "folkloric" stories and accounts stemmed, however, from attempts at clerical instruction (*exempla*) or suppression (inquisition), which has clearly influenced the interpretation of Latin Christian culture as essentially "repressive." Crucial to the argument of this school is the conviction that these stories, despite stereotyping, repetition, and elaboration, preserve a core of authentic experience and belief alien to the Christian establishment. However true that may be—and it is ultimately unverifiable except by using structural or comparative analysis—the impulse to compile exemplary stories and inquisitorial records originated with learned clerics, making these sources at best only a very indirect indicator of the strength and quality of popular folklore and a much better indicator of clerical zeal to deepen Christian faith and practice. Even Schmitt conceded that the uncovering of strange happenings in a remote village suggests a deep penetration of Christianity into the countryside.[47] Moreover, Alexander Murray, in a recent study of this same genre, found stories of plenty of people who struggled with moral and cultic questions pertinent to Christian practice but accounts of only one or two who stood altogether outside or against medieval Christendom.[48] Similarly, new studies using the sermon literature of the thirteenth century—a collection of documents that underwent enormous expansion then and within which *exempla* are merely a subcategory—have yielded much information on common practices and beliefs. Those texts reveal an outlook shared, more or less, between preacher and audience, even when the audience—not so surprisingly—might sometimes have appeared terribly sinful or even maddeningly indifferent.[49]

These sources comprise a relatively meager harvest of materials. From them historians can produce a separate religious culture only by inferring from selected stories—admittedly, with striking parallels to Indo-European folklore—a supposed majority culture and by assuming that this culture remained essentially unchanged from prehistoric times to the late Middle Ages and beyond. This is no less dogmatic and ahistorical in its interpretive approach than the neo-Scholastic Catholic who inferred from Bernard or Thomas a thriving Christian culture across the whole sweep of the Middle Ages. But this assumption of two cultures, including

of sources, see Leonard Boyle, "Montaillou Revisited: 'Mentalité' and Methodology," in J. A. Raftis, ed., *Pathways to Medieval Peasants* (Toronto, 1981), 119–40.

[46] Jacques Le Goff and Jean-Claude Schmitt, "Au XIII^{e} siècle: Une parole nouvelle," in Delumeau, *Histoire vécue*, 271–75. Essays by Pierre Riché, C. M. de la Roncière, Alexandre Geystor, and François Rapp, each with references to additional works, also recognize, but in a more balanced way, a folk religion; *ibid.*, 195–221, 281–314, 315–34, 335–64.

[47] Schmitt, *The Holy Greyhound*, 177. In the *L'exemplum*, Schmitt spoke, with greater nuance, of an effort to "reinforce the process of Christianization"; *L'exemplum*, 102–03.

[48] Alexander Murray, "Confession as a Historical Source in the Thirteenth Century," in Davis and Wallace-Hadrill, *The Writing of History*, 275–322.

[49] Alexander Murray, "Piety and Impiety in Thirteenth-Century Italy," in G. Cumming and D. Baker, eds., *Popular Belief and Practice* (Cambridge, 1972), 83–107, and "Religion among the Poor in Thirteenth-Century France: The Testimony of Humbert of Romans," *Traditio*, 30 (1974): 285–324; and, especially, D. L. D'Avray, *The Preaching of the Friars: Sermons Diffused from Paris before 1300* (Oxford, 1985).

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a silent majority largely untouched by society's religious leaders for nearly a thousand years, has not gone unchallenged. Recent studies of the cults of the saints and relics—the point of origin for many studies of popular religion—have rejected any such notion of a dual or bilevel religious culture. Gregory the Great wrote his *Dialogues* to instruct and please Roman aristocrats, not just to pander to peasants; Gregory of Tours described peasants and bishops alike attempting to gain access to the holy power of relics; and Carolingian reliquary shrines attracted the veneration of learned monks and bishops as well as lords and peasants.[50] There certainly was much more to medieval religious culture than a naive reading of the prescriptive sources might suggest, but medieval Christianity is better conceived as comprised of complex and diverse elements spread across a very wide but more or less continuous spectrum. The burden of proof still lies with those who argue for two radically different religious cultures. For they must then also demonstrate—and not just assume—a disjunction between popular practice and ecclesiastical institutions, between "low" and "high" culture, so great and so absolute as to exclude any appreciation of mutual influence, thus driving a very deep and quite improbable wedge between religious mentalities and religious institutions. Yet this thesis has probably done more than anything else in the last two decades to make religious culture, encompassing all the people and all aspects of medieval society, central to medieval historiography.

As historians succeeded in bringing religious life into the mainstream of medieval history, interpretations of medieval Christianity inevitably fell subject to the same forces or predispositions affecting historiography generally since the Second World War. The resultant impact on our conception of a "Christian Middle Ages" may be illustrated in three general tendencies or emphases.

First, historical study has become particularized and localized. Only forty years ago a French author could begin a book on the "medieval Christian empire" containing the declaration that medieval Christendom offered a "stable climate," a "unified whole (*bloc*)" within which "medieval men lived and moved." An Italian author of the same era referred to a "Holy Roman Republic" that included a "universalism of church and empire, of Romanism and Christianity [that] excluded any possibility of civilized life outside itself." Only a generation later such views seem illusory. This thousand-year stretch of history produced a variety of Christian ideals and societies or, if one prefers, of "Christendoms."[51] Such

[50] Adalbert de Vogué, *Grégoire Le Grand, Dialogues*, Sources chrétiennes, no. 251 (Paris, 1978); Peter Brown, *The Cult of the Saints: Its Rise and Function in Latin Christianity* (Chicago, 1981); and J. Hubert and M.-C. Hubert, "Piété chrétienne ou paganisme? Les statues-reliquaires de l'Europe carolingienne," in *Cristianizzazione ed organizzazione ecclesiastica delle campagne nell'alto medioevo: Espansione e resistenze* (Spoleto, 1982), 235–68. For Brown's own change of outlook, see his "The Saint as Exemplar in Late Antiquity," *Representations*, 1.2 (1983): 1–25.

[51] Henri Brémond, *L'empire chrétien et ses destinées en occident du XI^e^ au XII^e^ siècles* (Paris, 1944), 10–22; and Giovanni Falco, *The Holy Roman Republic: A Historic Profile of the Middle Ages*, trans. K. Virginia Kent (New York, 1964), 22. By way of contrast, see Delumeau, "Au sujet de la déchristianisation," 56. Similarly, in secular history, Georges Duby's inaugural lecture at the Collège de France spoke of "des

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particularization, altogether expected in regional studies, has also begun to affect our understanding of broader subjects, in particular, Christian religious organization and culture. Scholars have shown, for instance, that "territorial" churches, "episcopalist" in structure and with distinct traditions in law, liturgy, and devotional practice, predominated at least to 1050, and in the later Middle Ages "national" churches emerged strengthened from the crisis of the Great Schism. Indeed, one scholar said that interventions from Rome and enhanced papal authority during the Gregorian Reform "ruined" the church in the province of Narbonne.[52] In monastic history, too, the "Benedictine centuries" and "Cluniac reform" have dissolved into a variety of competing Benedictine enterprises,[53] which has resulted in a new perspective on the innovative Cistercian effort to reform by centralization and institutionalization. In philosophy, scholars over the last generation have rediscovered the influence of Platonic thought as well as the importance of differing receptions of Aristotle; in theology, many different schools of thought, usually linked to particular universities or religious orders, have replaced any vague notion of a single or continuous "Scholastic" tradition.[54] Even the Christian ideal itself, as represented by its saints, has been shown to differ widely in space (north and south of the Alps) and in time (eighth-century Aquitaine versus thirteenth-century Lombardy).[55] In sum, few medieval historians speak with any confidence of some common ideal animating all of medieval Christianity irrespective of particular places, times, schools, orders, and authors.

sociétés médiévales," not "la société médiévale." The lecture was reprinted as "Les societés médiévales: Une approche d'ensemble," in his *Hommes et structures du moyen âge* (Paris, 1973), 361–79.

[52] The early medieval "episcopalist" church has been much studied since the Second World War but was first explicated by Theodor Schieffer. See Schieffer, "La chiesa nazionale di osservanza romana," in *Le chiese nei regni dell'Europa occidentale e i loro rapporti con Roma sino all'800* (Spoleto, 1960), 73–94. Also see Fredrich Kempf, "Chiesa territoriali e chiesa romana nel secolo VIII," in *I problemi dell'occidente nel secolo VIII* (Spoleto, 1973), 293–317; and Horst Fuhrmann, "Das Papsttum und das kirchliche Leben im Frankreich," in *Nascità dell'Europa ed Europa carolingia: Un'equazione da verificare* (Spoleto, 1981), 419–56. For surveys of the late medieval church, see J. A. F. Thomson, *Popes and Princes, 1417–1517: Politics and Polity in the Late Medieval Church* (London, 1980); Francis Oakley, *The Late Medieval Church* (Ithaca, N.Y., 1979), 23–79. On the church in Narbonne, see Elizabeth Magnou-Nortier, *La société laïque et l'église dans la province écclésiastique de Narbonne de la fin du VIII^e^ à la fin du XI^e^ siècle* (Toulouse, 1974).

[53] See Kassius Hallinger, *Gorze-Kluny*, Studia Anselmiana, nos. 22–25 (Rome, 1950–51). For an appreciation of Hallinger and a discussion of the context of his work, see Giles Constable, "The Study of Monastic History Today," in Constable, *Religious Life and Thought (11th–12th c.)* (London, 1979), chap. 1.

[54] On Platonism, see the work of Stephen Gersh, for instance, his *From Iamblichus to Eriugena* (Leiden, 1978). For a pioneering essay, see M.-D. Chenu, "The Platonisms of the Twelfth Century," in his *Nature, Man, and Society*, 49–68. On Aristotelianism, see Ferdnand van Steenberghen, *La philosophie aux XIII^e^ siècle* (Louvain, 1966), together with the essays and bibliography in the *Cambridge History of Later Medieval Philosophy*. On theology, see, for instance, Heiko Oberman, "Fourteenth-Century Religious Thought: A Premature Profile," *Speculum*, 53 (1978): 80–93; William Courtenay, *Covenant and Causality in Medieval Thought* (London, 1984); and J. I. Catto, "Theology and Theologians, 1220–1320," in Catto, *The History of the University of Oxford*, vol. 1, pp. 471–517.

[55] Among many works, see Wilson, *Saints and Their Cults*, esp. 309–417; Joseph-Claude Poulin, *L'idéal de sainteté dans l'Acquitaine carolingienne d'après les sources hagiographiques, 750–950* (Quebec, 1975); André Vauchez, *La sainteté en occident aux derniers siècles du moyen âge* (Rome, 1981); Michael Goodich, *Vita perfecta: The Ideal of Sainthood in the Thirteenth Century* (Stuttgart, 1982); and Donald Weinstein and Rudolph Bell, *Saints and Society: The Two Worlds of Western Christianity, 1000–1700* (Chicago, 1982).

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Second, widespread enthusiasm for sociological interpretations of historical phenomena has also had its impact on our comprehension of medieval religious life. Vague talk about the "spirit," "genius," or underlying "faith" of medieval Christendom has given way to depictions of a church that functioned virtually as a "state" with a strong sense of its own socioeconomic and political interests, of religious houses that acted as major socioeconomic forces in their regions, and of a papacy seated firmly in its social setting in Rome. From one of Robert Brentano's studies, one could easily conclude that the thirteenth-century church's most important figure was not a pope, theologian, or mystic but the proctor, the facilitator of endless routine business.[56] A series of contrasts may help make plain the dimensions of this historiographical shift. Where Hippolyte Delehaye attempted to place medieval hagiography in a larger spiritual and literary tradition, Vauchez and many others have concentrated on its social outlook and setting.[57] Where Walter Ullmann ascribed the papacy's ascendancy to its inherent legal and theological principles, Jeffrey Richards, Jane Sayers, and many others have traced the rise of papal authority to sociopolitical and administrative developments in Rome and the curia.[58] Where Grundmann ascribed the twelfth-century religious revival to a quest for the apostolic ideal, a whole generation of scholars has focused on varying reactions to the so-called Commercial Revolution, whence the reformers' troubled fascination with religious poverty.[59] Where Grundmann again ascribed the rise of universities to a quest for learning, Southern, Peter Classen, and John Baldwin, hardly social determinists, have stressed the function of university men in society and the development of a new "learned elite" (*Bildungsstand*).[60] And where an earlier generation of historians of theology was endlessly fascinated with uncovering the intellectual roots of Scholasticism, more recent scholars have taken up such "social" questions as the sacramentality of

[56] Southern, *Western Society and the Church*, 15–23; Pamela Johnson, *Prayer, Patronage, and Power: The Abbey of La Trinité, Vendôme, 1032–1187* (New York, 1981); Jeffrey Richards, *The Popes and the Papacy in the Early Middle Ages, 476–752* (London, 1979); and Brentano, *Rome before Avignon: A Social History of Thirteenth-Century Rome* (New York, 1974), *Two Churches: England and Italy in the Thirteenth Century* (Princeton, N.J., 1968).

[57] Delehaye, *Sanctus: Essai sur le culte des saints dans l'antiquité* (Brussels, 1927); and Baudouin de Gaiffier, *Recherches d'hagiographie latine* (Brussels, 1971). And, for a work still mainly literary and religious in approach, see Richard Kieckhefer, *Unquiet Souls: Fourteenth-Century Saints and Their Religious Milieu* (Chicago, 1984). Important examples of works that follow the sociological approach are listed in note 55, above.

[58] Walter Ullmann, *The Growth of Papal Government in the Middle Ages: A Study in the Ideological Relations of Clerical to Lay Power* (London, 1955). Also see Francis Oakley, "Celestial Hierarchies Revisited: Walter Ullmann's Vision of Medieval Politics," *Past and Present*, 60 (1973): 3–48; and Friedrich Kempf, "Die päpstliche Gewalt in der mittelalterlichen Welt: Eine Auseinandersetzung mit Walter Ullmann," *Miscellanea Historiae Pontificiae*, 21 (1959): 117–69. Compare Richards, *The Popes and the Papacy*; Jane Sayers, *Papal Government and England during the Pontificate of Honorius III (1216–27)* (Cambridge, 1984); and Cheney, *Innocent III and England*.

[59] For a summary of many works, see Lester K. Little, *Religious Poverty and the Profit Economy* (Ithaca, N.Y., 1978); and Lester K. Little and Barbara Rosenwein, "Social Meaning in the Monastic and Mendicant Spiritualities," *Past and Present*, 63 (1974): 4–32.

[60] Herbert Grundmann, "Vom Ursprung der Universität," in *Ausgewählte Aufsätze*, 3: 292–342. Contrast Peter Classen, *Studium und Gesellschaft*, ed. J. Fried, Monumenta Germaniae Historica, Schriften, no. 29 (Stuttgart, 1983); John Baldwin, "Masters at Paris from 1179 to 1215: A Social Perspective," in Benson *et al.*, *Renaissance and Renewal*, 138–72; and Southern, "The Schools of Paris and the School of Chartres," and "From Schools to University."

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marriage and such "practical" matters as the development of pastoral care.[61] All this is not even to discuss a host of studies on the social composition of individual religious houses, dioceses, and so on—for example, recent attempts to apply the modern sociological concept of "anomie" to the conditions in which Cluny arose or to move beyond the theoretical dimensions of the Franciscan dispute over the issue of poverty by analyzing the actual wealth and social position of one city's mendicant houses.[62] Indeed, scholars are now using religious data to draw social inferences: the existence of a "city" from its mendicant foundations, familial and social relationships from hagiographical accounts, social "mentalities" from shifting attitudes toward Beguines and Beghards, and even a "sociological" interpretation of the mass.[63]

The third general trend in recent historiography is its dramatic shift downward. For studies of medieval religious life, this means the examination of "popular religion" now threatens to eclipse work on popes, theologians, and bishops.[64] The impact of this shift is most evident in the themes chosen for research. Doctrinal disputes and papal policies have given way to relics, the cult of the saints, pilgrimages, miracles, purgatory, and the like. Even Rosalind Brooke and Christopher Brooke, moderate scholars on this question, judged that it was appropriate to begin their recent work with relics, pilgrimages, and the cult of the saints before concluding with the sacraments and teachings of Scripture.[65] Only thirty-five years ago, Christopher Dawson in his Gifford lectures found true Christianity much more deeply rooted among the people than among a corrupt clerical elite.[66] Today, on the other extreme, there is a curious and quite unjustified tendency, as in LeRoy Ladurie's *Montaillou*, to identify the "popular" with the

[61] Hans Zeimentz, *Ehe nach der Frühscholastik* (Dusseldorf, 1973); and Leonard Boyle, *Pastoral Care, Clerical Education, and Canon Law, 1200–1400* (London, 1981), and "*Summae confessorum*," in *Les genres litteraires dans les sources théologiques et philosophiques médiévales* (Louvain, 1982), 227–37.

[62] Barbara Rosenwein, *Rhinocerous Bound: Cluny in the Tenth Century* (Philadelphia, 1982); and B. Neidiger, *Mendikanten zwischen Ordensideal und städtischer Realität: Untersuchungen zum wirtschaftlichen Verhalten der Bettelorden in Basel* (Berlin, 1981).

[63] Jacques Le Goff, "Apostolat mendiant et fait urbain dans la France médiévale: L'implantation des ordres mendiants," and "Ordres mendiants et urbanisation dans la France médiévale: Etat de l'inquète," *Annales, économies, sociétés, civilisation*, 23 (1968): 335–52, and 25 (1970): 924–46; John Freed, *The Friars and German Society* (Cambridge, Mass., 1977); Bell and Weinstein, *Saints and Society*; Schmitt, *Mort d'une hérésie*; and John Bossy, "Essai de sociographie de la messe," *Annales, économies, sociétés, civilisation*, 36 (1981): 44–70. For a review of Schmitt's *Mort d'une hérésie* by Robert Lerner and one by Lawrence Duggan, see, respectively, *Speculum*, 54 (1979): 842–44 and *AHR*, 84 (1979): 1034–35.

[64] For orientation to a vast number of new works, see Manselli, *Religion populaire*; Rapp, "Reflexions sur la religion populaire;" and Natalie Davis, "Some Tasks and Themes in the History of Popular Religion," in Charles Trinkaus and Heiko Oberman, eds., *The Pursuit of Holiness in Late Medieval and Renaissance Religion* (Leiden, 1974), 307–36.

[65] Jonathon Sumption, *Pilgrimage: An Image of Medieval Religion* (Totowa, N.J., 1976); Barbara Ward, *Miracles and the Medieval Mind* (Philadelphia, 1982); and Brooke and Brooke, *Popular Religion in the Middle Ages* (New York, 1983).

[66] Christopher Dawson, *Religion and the Rise of Western Culture* (London, 1950), 219: "And [Langland's] poem seems to prove that the fundamental principles of the creative period of medieval religion had been more completely assimilated and incorporated by the new vernacular culture of the common people than it had been by the higher and more literary culture of the ruling elements in Church and state." This is virtually the exact opposite of the views fashionable today. But Dawson also recognized that "the early centuries of the Middle Ages saw the rise of a new Christian mythology [meaning, the legends of the saints]" and that "it was inevitable that the Christian ascetic and saint should acquire some of the features of the pagan shaman and demigod"; *ibid.*, 34.

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"outlandish" or the "extraordinary." Studies of a "routine" religious life at lower social levels that was neither learned nor extraordinary nor a vehicle of protest are just beginning to make their appearance. Yet what some of the first of these revealed of ignorant superstition and indifferent sacramental practice probably did more than anything else to tarnish the old Romantic image of medieval Christianity. One recent study, on the other hand, based on wills and combining all these new emphases on the local, the social, and the popular, discloses a vigorous Christian practice shared by both clerics and laity in late medieval Norwich and argues that the Reformation was built there on strength rather than on failure.[67] While the inclusion of new approaches to medieval Christianity has certainly done much to undermine the image of it created by reading the accomplished texts of monks and mendicants, the eventual integration of this material will actually deepen our understanding of the way in which religious teachings and institutions shaped ordinary lives, and ordinary lives in turn those teachings and institutions.

A brief word should be said about the way in which the introduction of religious life into the mainstream of medieval historical inquiry has intersected with traditional confessional considerations. A topic such as medieval Christianity has inevitably—and quite understandably—attracted many historians with distinct confessional interests. Change over the last generation or so within the Christian church has had a noticeable affect on the study of medieval Christianity. The rapprochement between Catholics and Protestants broadly called "ecumenism" has not altogether blurred their differing perspectives, but it has decreased the tendency to adopt interpretations derived more from religious affiliation than from historical inquiry. Protestant as well as Jewish and agnostic historians now approach medieval religious life far more sympathetically, and Catholic historians in turn far more critically, than was conceivable in the past. At times inherited roles and outlooks have been virtually reversed. Plainly put, medieval Christianity is no longer an overwhelmingly "Catholic" subject attracting mainly "Catholic" scholars.

Great changes have come within the Roman Catholic Church where traditionally the largest number of medieval religious historians have been found. These Catholic scholars, perceptibly caught up in all the issues that arose from the Second Vatican Council, have done as much as any other group to change our understanding of medieval Christianity. At the risk of oversimplifying, three tendencies seem most apparent. First, where an earlier generation saw medieval Christianity as a complete entity or as "development of doctrine," recent scholars, led by Catholics such as Gerhart Ladner, have approached it as a series of reform movements, pointing up not only the centrality of reform both as an idea and an impulse but also, implicitly, the recurring need for reform. Second, where an earlier generation saw the medieval church as quintessentially papal, recent scholars, led by Catholics such as Brian Tierney, Francis Oakley, Congar, and

[67] Jocelyn Hillgarth, "Popular Religion in Visigothic Spain," in E. James, ed., *Visigothic Spain: New Approaches* (Oxford, 1980), 3–60; Toussaert, *Sentiment religieux en Flandre*; and Norman P. Tanner, *The Church in Late Medieval Norwich, 1370–1532*, Studies and Texts, no. 66, (Toronto, 1984). Also see François Rapp, *Réformes et Réformation à Strasbourg: Eglise et société dans la diocèse de Strasbourg, 1450–1525* (Paris, 1974).

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many others, have discovered its conciliar dimension, predominant in practice in the early and late Middle Ages and vital in theory even during the strongest papal period. And, third, where an earlier generation located medieval Christianity's main intellectual achievement in a kind of philosophical theology, recent scholars, led by Catholics such as Smalley, Henri De Lubac, van Steenberghen, Stephen Kuttner, and Ullman, have sought that achievement in biblical theology, philosophical pluralism, and ecclesiastical jurisprudence. Add what I have said about Le Bras's emphasis on local practice, Delaruelle's on lay piety, and Manselli's on dissenting religions, and it is clear that Catholics themselves have radically redone the Ultramontane, neo-Scholastic image of medieval Christianity on which they were reared only a generation ago.

ALL IN ALL, TO PARAPHRASE A LINE from Lynn White, Jr., one of my teachers, medieval Christianity is not what it used to be. This is less the result of new findings (important as they are) than of new questions. In an age frequently labeled "post-Christian," in which secular governments must spend large sums of money to preserve "redundant" churches as museum pieces, scholars and other people alike have begun to wonder how the European countryside ever came to be dotted with so many churches, how clerics came to be counted as the "first order" in society, and how culture came to be shaped and censored by religious norms. Was this not all imposed by an ambitious "clerical elite"? Is not secularization or "de-Christianization" simply a return to the views most people have always held? Were not most medieval folk far less caught up in Christian culture than has been assumed? In their boldness and bluntness, these questions can seem almost rhetorical. So much older work on medieval culture then suddenly seems beside the point, particularly if scholars are prepared to dismiss that culture as the product and outlook only of a minuscule elite. The question, in the jargon of our day, is the extent of the "Christianization" of medieval society, that is, the degree to which specifically Christian teachings and practices shaped the cultural milieu of medieval folk both high and low.[68] It is a question fundamental to the interpretation of European medieval culture.

Long before the Restoration Romantics, most Europeans took for granted the formative influence of Judeo-Christian religion in the shaping of European culture. And Le Goff, building on the work of Michelet, emphatically declared the medieval epoch crucial to the formation of a distinctive European civilization—mentioning every area except religion. What is needed, then, is a way of getting at the truth about medieval religious life and of stating the reality of its imprint and legacy without falling prey to either extreme, a mythical golden age of Catholic Christianity or an equally mythical millennium of Indo-European folk religion. Scholars must recognize, as reflective medieval folk did, that medieval religious life included a constant struggle to establish or renew Christian religious culture in the

[68] "Christianization" has become something of a catchword now, and it would be futile to list all the books that refer to it. But see, for instance, Jean Delumeau, *Un chemin d'histoire: Chrétienité et déchristianisation* (Paris, 1981); *Cristianizzazione ed organnizzazione*; and Ramsey MacMullen, *Christianizing the Roman Empire (A.D. 100–400)* (New Haven, Conn., 1984).

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face of various other religious practices, some derived from what Powicke described as the "paganism of the natural man," some of more ancient heritage, as Le Goff and Schmitt would rightly insist. Some of these practices and outlooks were rejected outright, others absorbed, even "baptized." The struggle was a real one and did not end, as older interpretations sometimes imply, with the official conversion of a people.[69] Medieval folk understood well enough that they did not live in a golden age of Christian religious life, though they constantly invoked Christian norms and practices as the ideal toward which they strove. Indeed, medieval churchmen acted so forcefully at times to repress dissent and superstition precisely because they understood just how tenuous Christianity's hold was and believed people's eternal fate truly hung in the balance (to borrow an image common on sculpted church fronts).

No single short essay could hope to survey all the recent work or even the basic problems pertinent to the question of Christianization, for in one sense it touches on nearly all aspects of medieval cultural history and much institutional history as well. The question is in fact much too broad, much too ill defined, and as such it will never receive a satisfying answer. The question must be brought into sharper focus, and what can be done in a brief essay is to direct attention toward six key issues or problems that bear scrutiny. But, first, two caveats.

Apart from the Indo-European folkloric tales accentuated by Le Goff, Schmitt, and others, most of the evidence for a failure of Christianization in the medieval period has come from accounts of incompetent clerics, ignorant peasants, superstitious women, infrequent communion, and the like.[70] Destructive as these accounts may be to the image of a "golden age" of medieval Christendom, they hardly prove that medieval culture was "non-Christian." To offer a simplistic counterexample, the fact that in the past election many Americans had only a superficial understanding of the issues and that barely half of them voted hardly proves that half of American voters are "monarchists" or "anarchists." Medieval Christian culture in fact saw and attempted to explain such lapses: increased demonic attacks on the advancing kingdom of the saints, the sinfulness of mankind, the differing levels of devotion expected of saints, clergy, and laymen, and so on. Moreover, that a record of such lapses exists is the result at least as much of intensified efforts at reform as of laments over failure. Whether similar evidence points up failure or efforts at reform has too often been a largely subjective matter. But historians should not discount the reality and actual degree of failure or the evidence of outright rejection. We must take seriously numerous reports of non-Christian practices, of peasants returning to older charms in moments of crisis, of princes and clerics tempted to join the Muslim or especially the Jewish faith, of scholars attracted by another vision of cosmology and causation. No doubt these phenomena received too little attention in the past. But to concentrate on

[69] Le Goff, *Time, Work, and Culture*, x–xi; and Powicke, *Christian Life*, 8: "The history of the Church is the record of the gradual and mutual adaptation of Christianity and paganism to each other. The complete victory of the former has always been a remote vision."

[70] See Toussaert, *Sentiment Religieux en Flandre*; Adam, *Vie paroissiale*; Murray, "Piety and Impiety"; and Delumeau, *Le Catholicisme*.

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them alone risks error as well by overlooking the larger cultural context in which clerics saw fit to record these instances as part of their continuing effort to instill Christian practices.

Second, if the new critics of a golden age of medieval Christendom have made some unwary assumptions in their use of evidence, so too have both old and new proponents of a Christian Middle Ages in their sense of Christianity's chronological advance. The basic scheme—more often assumed than stated or argued—rests on a vague sense of evolutionary development—so frequently applied in the nineteenth century to social, political, and religious events. The process of Christianization was conceived as beginning after the collapse of the Roman empire with the mass baptism of Germanic, Celtic, and Slavic peoples whose grasp of Christian doctrine and practice must have been embryonic at best. The process then advanced to an effective implanting of Christianity during the Carolingian era (and about the year 1000 on the northern and eastern peripheries of Europe), a first real "taking root" in the period of the reform movements (1050–1150), and a flowering from the time of Francis (d. 1226) and Lateran Council IV (1215), followed at some point in the fourteenth century by decay or, in the image projected by Johan Huizinga's famous book, a kind of "autumnal over-ripeness." A newer scheme, which attempts to include religious culture within the interpretive schemes of social history favored by the *Annales* and related schools, accepts the first three stages with some reserve, then puts the real turning point in the process of Christianization in the later twelfth century, followed by relative continuity in religious practice until much later in the history of "Old Europe" or the *ancien régime*. Neither scheme adequately takes account of great regional and cultural variations over a period of one thousand years. The point here is not to reject these schemes as wholly false or to repudiate all such schemes but to register a warning about the arbitrariness of their imposition. The word "Christianization" suggests an unfolding process, and it is all too tempting to fill in the stages of development from too little evidence or to fix the temporal boundaries of each stage. Historians must accept that there were periods of sporadic change, when a pattern of advance or regression is not perceptible, periods of discernible change whether for good or ill, and periods of relative stability, without the whole necessarily fitting into a continuing, evolutionary process.

There follow, then, six observations on Christianization, understood as an object of historical study and as an essential part of the medieval cultural legacy. First, an examination of the word and the concept. Unlike so many historical labels, such as feudalism, "Christendom" (*Christianitas*) was a term medieval writers applied to themselves and their civilization. What they understood by it has received too little attention. Manselli ascribed the origin of the concept and the term to a self-conscious, defensive reaction to Islam. Most other scholars have focused their attention primarily upon uses of the term in reference to ecclesiastical polity, that is, Christendom as a universal society of believers subject to the vicar of Christ in Rome, with all that implies for the relations of "Christians" to kings and of

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"Christian kings" to popes.[71] Both ideas represent one part of the truth, but there is another more basic meaning that is especially pertinent here and found as well in vernacular texts.

King Edmund's *Code*, issued in the 940s, enjoined tithes on "every Christian man by reason of his Christianity" (aelcun Christene men be his Cristendome), and in the prologue to his second code King Edmund spoke of taking council with people both high and low on how best to cultivate Christianity in his kingdom (hu ic maehte Cristendomes maest araeran). Laws from the early eleventh century, probably written by the cleric Wulfstan, spoke of "violating one's Christianity [*Cristendom*] or honoring heathendom [*haethendom*] in word or deed." And Ethelred began his law code with a common pledge to love God, to hold zealously to a single Christianity (*aenne Cristendom*), to cast off all paganism (*aelcne haethendome*), and to hold to one Christianity under one kingship (thaet we under anum cynedome aenne Cristendom healdan willath). A Latin version of Ethelred's code, containing various nuances and amplifications, speaks of "Christ-worshippers [*Christicolarum*]," the "observance of the Catholic religion" (catholice cultu religionis), and a "Christian religious practice [cultu religionis Christianae] wholly free of superstition"—all synonyms, it seems, for *Christianitas*.[72]

The meaning of *Christianitas* in the vernacular is clear enough from these texts: a common religious observance (*cultus*) overseen and enforced by the king together with his lords and bishops. Observance began with baptism, the simplest meaning of *Christianitas*, as in the English word "christening." This society of the baptized could assume a vague territorial sense—quite apart from the threat of Islam—as when anyone who dared marry a nun was declared "an outlaw before God, banned from 'Christendom,' and forfeit of his properties."[73] But a "ban from Christendom" meant first of all a ban from the sacraments, the heart of religious practice, for in Latin the term *Christianitas* covered all the sacramental and liturgical ministrations suspended by an interdict. Thus, one English chronicler could laconically report that, "on the order of the pope, Christianity was prohibited." Scholastic theologians did not lose sight of the most basic meaning of *Christianitas*. Albertus Magnus declared a Christian "perfect [or complete] whose Christianity sufficed for salvation" (perfectus Christianus est, cui Christianitas sufficit ad salutem),[74] by which he meant a person whose faith and practice, liturgical and moral, sufficed for salvation. *Christianitas* could also be a term for

[71] Raoul Manselli, "La *respublica christiana* e l'Islam," in *L'Occidente e l'Islam nell'alto medioevo* (Spoleto, 1965), 115–47, and "Christianitas," in *Lexikon des Mittelalters*, vol. 2, 1915–16. For interpretations focusing on ecclesiastical polity, see Jean Rupp, *L'idée de la chrétienté dans la pensée pontificale des origines à Innocent III* (Paris, 1939); Gerhart Ladner, "The Concepts of 'Ecclesia' and 'Christianitas' and Their Relation to the Idea of Papal 'Plenitudo potestatis' from Gregory VII to Boniface VIII," in *Sacerdozio e regno da Gregorio VII a Bonifacio VIII* (Rome, 1954), 49–77; Jan van Laarhoven, "'Christianitas' et réforme grégorienne," *Studi Gregoriani*, 6 (1959/1961): 1–98; and Friederich Kempf, "Das Problem der Christianitas im 12.-13. Jahrhunderten," *Historisches Jahrbuch*, 79 (1960): 104–23.

[72] Dorothy Whitelock *et al.*, *Councils and Synods* (Oxford, 1981), 1: 62, 65, 305, 344, 362–63. Whitelock normally translated *Cristendom* as "Christian faith," which seems partly to miss the element of observance and practice that was actually so basic.

[73] *Ibid.*, 440.

[74] "Quibus iussu papae . . . christianitas prohibita est." *Annales Waverly*, ed. H. R. Luard, Rolls series, vol. 36.2, p. 229. Albertus *De sacramentis* 65, in *Opera Omnia*, vol. 26, pp. 45, 52. For more examples,

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such a person (*vestra christianitas*) both in Latin and in the vernacular. In French epic literature, *Christientet* was probably used most often to distinguish Christians from Saracens, but what underlay that distinction was an evident set of beliefs and especially practices. Dante had *cristianesmo* rhyme once with *battesmo* and *battesmo* rhyme twice with *paganesmo*, establishing the same verbal and conceptual associations.[75] In its most basic sense, then, the word *Christianitas* referred neither to an ecclesiastical polity nor to the opponents of Islam but to the religious faith and practice that medieval men entirely presupposed when elaborating their political policies and philosophical propositions. Or, as one of the earliest Dutch-Latin lexicons put it at the end of the Middle Ages (ca. 1480), "*Christianitas* is the rite and/or property [ritus vel proprietas] by which people are called Christian."[76]

This meaning also had an institutional manifestation—and not just in the sense of "all Christendom" or the whole society of believers in Europe. From the mid-ninth century, bishops appointed rural deans to oversee country pastors and their ministrations; especially after the later twelfth century, these deans came to be called "deans of *Christianitas*," their districts sometimes simply *Christianitates*, their courts "court Christian," and their meetings or rulings a "council [*concilium*] of *Christianitas*."[77] Such deans of Christianity had responsibility precisely for the way in which country priests carried out all the duties and rites essential to the maintenance of *Christianitas*, thereby insuring the Christian character of the people. The extant sources pertinent to these deans and the religious culture they supervised are few, partly because of the odds against preserving such local archival records and partly because much of the deans' supervision must have taken place in ways never committed to writing.[78] But the point still stands: the figure, beyond the local priest himself, with responsibility for religious culture in the countryside was called the "dean of *Christianitas*." "Christendom" was the term medieval folk at every level used to identify their religious culture.

Second, Christianization therefore required, in cultural terms, the putting in place of the means to structure Christian profession and practice, that is, the rituals

see the *Dictionary of Medieval Latin from British Sources* (London, 1981), vol. 2, p. 333; and *Mittellateinisches Wörterbuch* (Munich, 1971), vol. 2, pp. 554–55.

[75] *The Song of Roland*, ll. 431, 3164, 3980; and *Dictionnaire de l'ancienne langue française* (Paris, 1938), vol. 2, pp. 130–31. Dante *Inferno* 4.37, *Paradiso* 24.106, *Purgatorio* 22.91, and *Paradiso* 20.125. My thanks to Christopher Kleinhenz of the University of Wisconsin for calling Dante's passages to my attention.

[76] *Conflatus vocabularum*, cited in the *Lexicon Latinitatis Nederlandicae medii aevi* (Leiden, 1981), vol. 2, p. 1186.

[77] See Paul Hinschius, *Das Kirchenrecht der Katholiken und Protestanten in Deutschland* (1878; reprint edn., Graz, 1959), vol. 2, pp. 269–77. For older works, see Hans E. Feine, *Kirchliche Rechtsgeschichte: Die katholische Kirche* (5th edn., Cologne, 1972), 201, 203–04, 427–28. Compare Paul R. Hyams, "Deans and Their Doings: The Norwich Inquiry of 1286," in Stephen Kuttner and Kenneth Pennington, eds., *Proceedings of the Sixth International Congress of Medieval Canon Law* (Vatican City, 1985), 619–46. The vocabulary can be followed through the standard medieval Latin dictionaries, such as those mentioned in notes 74 and 76, above.

[78] For a summary of the pertinent Carolingian legislation, see Regino of Prüm *De synodalibus causis et disciplinis ecclesiasticis*. One interesting set of records from the fourteenth century and the diocese of Liège has been published; "Droit coutumier du Concile de Bastogne," *Annales de L'Institut Archéologique du Luxembourg*, 58 (1927): 16–26. Also see Hyams, "Deans and Their Doings"; and Jean Burcklé, *Les chapitres ruraux des anciens éuêchés de Strasbourg et de Bâle* (Colmar, 1935).

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and institutions deans and priests supervised.[79] The official christening of a king or people hardly produced deep faith or organized practice overnight; in anthropological terms, it did not in itself transform a customary culture. Two Spoleto conferences (often good barometers of historical interests), less than fifteen years apart, nicely addressed this shift in historiographical perception. The first dealt with the conversion to Christianity in Europe; the second was entitled "Christianization and Ecclesiastical Organization of the Countryside: Advances and Resistance," thereby getting at what was actually required for "conversion of Europe."[80]

In the early Middle Ages, observance of Christian ritual and practice normally preceded local institutional organization, often by generations. Until the year 1000, it remained common for people simply to assemble from time to time around a cross. But eventually and at varying times throughout Europe, the parish church emerged as the seat of ritual and practice. Recently, scholars have begun to place great emphasis on the parish as one of the fundamental institutions of medieval and Old Europe, but these studies have also demonstrated its relatively slow and late formation. Basic parish organization was not complete until the eleventh or twelfth century, even in Italy, where Christianity first reached Europe, and parishes developed correspondingly later as Christianity moved toward the northern and eastern peripheries of Europe.[81] Well into the eleventh century, "parish" (*parochium*) usually still meant "diocese," and "priest" (*sacerdos*) meant the bishop. Most country parishes, by contrast, were formed at local initiative, that is, most grew slowly out of the needs or interests (spiritual as well as financial) of local communities and almost never from any hierarchical masterplan, although some churches certainly were established by force, for example, by Charlemagne's Saxon campaigns. Local formation of parishes created the nearly insoluable problem of "private" or "proprietary" churches with a mixture of religious, political, and economic motives quite impossible to disentangle. A sense of territoriality increased only slowly and primarily as a result of Carolingian tithing legislation.[82] But, in time, the way parishes developed locally across Europe had an enduring effect. The parish became the center of medieval village life.

[79] A simple description from a charter dated 1204 reads as follows: "Per sacerdotem eius capelle ibidem baptizentur parvuli, sepeliantur mortui, visitentur infirmi, introducantur nupte, et cetera que ad christianitatem pertinent debite peragantur." Klaas Heeringa, *Oorkondenboek van het Sticht Utrecht* (Utrecht, 1959), vol. 2, no. 568, p. 34.

[80] *La conversione al cristianesimo nell'Europa dell'alto medioevo* (Spoleto, 1967); and *Cristianizzazione ed organizzazione ecclesiastica*.

[81] For orientation to this complex subject, see Jean Gaudemet, "La paroisse au moyen âge," *Revue d'histoire de l'église de France*, 59 (1973): 5–21. For new recognition of the parish as a fundamental element of "Old Europe," see Susan Reynolds, *Kingdoms and Communities in Western Europe, 900–1300* (Oxford, 1984), 79–100; and Robert Fossier, *Enfance de l'Europe: Aspects économiques et sociaux* (Paris, 1982), 345–64. For newer work on parishes and religious life, see *Cristianizzazione ed organnizzazione*; Cinzio Violante, "Pievi e parrocchie dalla fine del X all'inizio del XIII secolo," in *Le istituzioni ecclesiastiche della "societas christiana" dei secoli XI–XII: Diocesi, pievi, parrocchie* (Milan, 1978), 643–799; and *Pievi e parrocchie in Italia nel basso medioevo (sec. XIII–XV)*, 2 vols. (Rome, 1984).

[82] Josef Semmler, "Zehntgebot und Pfarrtermination in karolingischer Zeit," in *Aus Kirche und Reich: Festschrift für Friedrich Kempf* (Sigmaringen, 1983), 33–44; Caroline Boyd, *Tithes and Parishes in Medieval Italy* (Ithaca, N.Y., 1952); and Jerzy Kloczowski, "Les paroisses en Bohème, en Hongrie et en Pologne (X^{e}–XIII^{e} siècle)," in *Le istituzioni ecclesiastiche*, 187–99. In Kloczowski's article, the Polish word for church is linked to the Latin word *castellum*.

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Anglo-Saxon, for instance, did not include a word for parish ("priest-shire" referred to the area of a priest's rule or responsibility), but "parish" in Middle English, almost from the beginning and certainly by the thirteenth century, could refer indiscriminately to village and church.[83] So, too, in French, the local commune (in recent studies treated as the *encadrement* of the countryside and folk) was most commonly known before the Revolution as the "parish." The village community found its identity in its "parish."

At the heart of parish life were its rituals. Synodical statutes reveal not only the practices normative for Christian observance but also, by implication and sometimes explicitly, the transgressions of that norm. Visitation records disclose what matters bishops, archdeacons, and their associates thought most in need of correction. Liturgical sources, though much harder for historians to deal with, are equally important. The real measure of Christian religious culture on a broad scale must be the degree to which time, space, and ritual observances came to be defined and grasped essentially in terms of the Christian liturgical year. Carolingian legislation placed great emphasis on Sunday as a required day of rest, thus punctuating the "secular" week and drawing attention to religious obligations. After the year 1200 or so, and much earlier in some instances, ordinary legal and administrative documents were routinely dated in terms of the appropriate saints' day or feast day. Of those rituals that touched each individual in the parish personally, the Christianization of birth and death seems to have proceeded rapidly in the ceremonies surrounding "christening" and "last rites."[84] Marriage, however, was not brought literally into the church and before the altar until the end of the Middle Ages, though Christian guidelines began to transform that institution in the ninth century and its sacramentality was established in the twelfth. The result, typically, was an overlay of Christian and other notions of marriage, but to imagine wholly separate and distinct conceptions of marriage competing over centuries is an exaggeration.[85] Similarly, private confession, which originated in the monastery, only gradually became a ritual practiced in the country parish, a routine rather than an extraordinary event required annually of every Christian after 1215. In the High Middle Ages it became the chief means of personal religious formation, chiefly by examining each individual against a standardized list of vices and virtues; even Dominican preaching aimed ultimately at bringing souls to confession. Two of the most sophisticated and successful pastoral manuals

[83] See "parish" in the *Oxford English Dictionary* (Oxford, 1933), vol. 7, p. 479.

[84] Jean Chelini, "La pratique dominicale des laïcs dans l'église franque sous le règne de Pépin," *Revue d'histoire de l'église de France*, 42 (1956): 161–74. For general orientation, see Arnold Angenendt, "Die Liturgie und die Organisation des kirchlichen Lebens auf dem Lande," in *Cristianizzazione ed organizzazione*, 169–226. On baptism and last rites, see, for instance, Arnold Angenendt, *Kaiserherrschaft und Königstaufe* (New York, 1984); and Damien Sicard, *La liturgie de la mort dans l'église latine des origines à la réforme carolingienne* (Munster, 1978).

[85] See J. B. Moulin and P. Mutembe, *Le rituel du mariage en France du XII^e au XVI^e siècle* (Paris, 1974); Suzanne Wemple, *Women in Frankish Society: Marriage and the Cloister, 500–900* (Philadelphia, 1982); and Zeimentz, *Ehe*. Contrast Georges Duby, *The Knight, the Lady, and the Priest* (New York, 1983). There is, once again, a good Spoleto volume; *Il matrimonio nella società alto medioevo* (Spoleto, 1977). For a short, but balanced, essay, see Christopher Brooke, "Marriage and Society in the Central Middle Ages," in B. Oathwaite, *Marriage and Society* (Baltimore, Md., 1983), 17–34.

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to come out of the Middle Ages, those by William of Pagula and Guido of Montrocher, both appeared in the early fourteenth century, and both are divided into three books that treat, respectively, the creed, commandments, and vices and virtues, the sacraments with particular emphasis on the eucharist, and penance. To highlight this last point is therefore appropriate, though to argue whether penance aimed at "religious formation" or "social control" is somewhat beside the point. Confession and penance shaped Christian faith and practice in all spheres of life, from the more overtly "spiritual" to matters of business (usury) and sexuality.[86]

However slowly and fitfully such ritual and juridical structures were put in place, they hold the key to the process of Christianization. The study of parochial organization, liturgy, confraternities, pastoralia, and so on will in time bring historians much closer to understanding "routine" religious life in the Middle Ages than the study of unusual instances, for example, of an isolated Cathar village, a holy dog, or an eccentric miller. Indeed, if the line of interpretation recently adopted by Bernd Moeller, Steven Ozment, Thomas Tentler, and other historians of the Reformation is correct, the problem by the late Middle Ages was of another order. The ecclesiastical culture and practices meant to Christianize the people, to shape their naming, marriage, formation, and burial, had so piled up as to become "burdensome," even insupportable, hence, the appeal of Luther's message of "liberty" for the Christian man.[87] This is not to argue that all medieval folk were deeply "Christianized" but only that the ecclesiastical and cultural mechanisms became, ironically, part of the problem.

Third, any study of religious culture must take religion seriously. Recent studies have made medieval religion important by treating it essentially in functionalist terms, setting out the ways in which religious culture dealt with human relations, illness, death, the fertility of fields and animals, and so on. Such an approach makes religion central and accessible without raising confessional issues. But it can easily become reductionist, thus ultimately unsatisfactory and even erroneous. Historians of religious culture must take "religious man" seriously, just as economic historians take "economic man," or political historians "political man," seriously.[88] Anything less will ultimately fail to do justice to the phenomena under investigation and, in particular, fail to account for the dynamic inherent in people acting on religious conviction.

[86] For the best approach to the enormous number of recent works, see Thomas Tentler, *Sin and Confession on the Eve of the Reformation* (Princeton, N.J., 1977); and Boyle, "*Summae confessorum*." For a debate between Tentler and Boyle, see *Pursuit of the Holy*, 103–37. Also see Roberto Rusconi, "De la prédication à la confession: Transmission et contrôle des modèles de comportement au XIII[e] siècle," in *Faire Croire*, 67–85. On William of Pagula, see Leonard Boyle, "The *Oculus Sacerdotis* and Some Other Works of William of Pagula," in his *Pastoral Care*, chap. 4. On Guido, see Louis Binz, *Vie religieuse et réforme écclésiastique dans la diocèse de Genève pendant le Grand Schisme et la crise conciliare (1378–1450)* (Geneva, 1973), 347–52.

[87] For a critical review of this interpretation, see Lawrence Duggan, "Fear and Confession on the Eve of the Reformation," *Archiv für Reformations geschichte*, 75 (1981): 153–75.

[88] For a similar point, but with an emphasis slightly different from mine, see Richard C. Trexler, "Reverence and Profanity in the Study of Early Modern Religion," in K. von Greyerz, ed., *Religion and Society in Early Modern Europe* (London, 1984), 245–69.

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In medieval Christendom, religious culture rested ultimately on "faith" or "belief," meaning professed assent to certain propositions as well as inner conviction. Those propositions were articulated in the Apostles' Creed, or, as it was most commonly called in medieval Latin, "the articles of the faith," and those articles were binding on all alike from peasant to pope. The first statutes prescribing the duties of priests, issued in the Carolingian era and then revived beginning in Paris shortly after 1200, more or less uniformly insisted that "all endowed with the name of Christian know the Lord's Prayer and the Creed."[89] Both the prayer and the creed were normally repeated at Sunday mass and by godparents at baptism; both were among the earliest texts translated into the vernacular languages. And such preaching as there was must have centered on them—together with the commandments, vices, and virtues—for they were given a basic exposition in nearly every known pastoral manual. So, too, nearly every inquisitorial proceeding began with some kind of question about adherence to the creed or "articles of the faith."

But medieval religious culture was also capable of a radical disjunction between knowledge and belief. The standard textbook in theology, compiled in the mid-twelfth century, noted that "there are those in the church who are less able, who cannot identify or distinguish the articles of the Creed; yet they believe what is contained in the Creed, for they believe what they do not know [credunt enim quae ignorant]." Out of this distinction developed the doctrine of an "implicit faith" held by the many "simple folk" who intended to believe what the church believed.[90] Both the doctrine and its varying application were of central importance in medieval religious culture. The authorized law book (and textbook) of the medieval church, Pope Gregory IX's Decretals of 1234, for example, begins with a profession of faith (taken from the first article of Lateran Council IV), thus making faith a matter of law. In an influential commentary, Pope Innocent IV (d. 1254) explained that all Christians were bound to believe that there is a God who rewards the good, and, implicitly, all Christians were bound to believe all the articles of faith held by the church. But priests charged with the cure of souls were to know the articles explicitly, though poor and unlearned minor clerics might also be dispensed from this.[91] In medieval religious culture, the element of cognitive

[89] "Ut praesbiteri praevideant ut omnes qui christiano censentur nomine orationem dominicam vel simbolum memoriter teneant," ed. Emil Seckel, *Neues Archiv*, 29 (1904): 292. Also see Peter Brommer, ed., *Capitula episcoporum I*, Monumenta Germaniae Historica (1984); and "*Capitula episcoporum*: Bemerkungen zu den bischöflichen Kapitularien," *Zeitschrift für Kirchengeschichte*, 91 (1980): 207–36. For an excellent discussion of the whole genre, see *"Capitula Episcoporum": Die bischöflichen Kapitularien des 9. und 10. Jahrhunderts*, Typologie des sources, no. 43 (Turnhout, 1985). For the early thirteenth century, compare the statutes from Paris: "Exhortentur populum semper presbyteri ad dicendam dominicam orationem et 'Credo in Deum' et 'Salutationem Beate Virginis," ed. Odette Pontal, *Les statuts synodaux français du XIII^e siècle* (Paris, 1971), vol. 1, pp. xxv–lxxvii, 74. For a good example of the genre's use to historians, see Joseph Avril, *Le gouvernement des évêques et la vie religieuse dans le diocèse d'Angers (1148–1240)*, 2 vols. (Paris, [1984]).

[90] Petrus Lombardus, *Sententiae*, Liber III, D. 25, c. 2 (3d edn., Grottaferrata, 1981), vol. 2, p. 155. Schmitt has recently given the doctrine of implicit faith more attention; "Du bon usage du *Credo*," in *Faire Croire*, 337–61.

[91] "De fide teneas quia quaedam est fidei mensura ad quam quilibet tenetur et quae sufficit simplicibus et forte etiam omnibus laicis, scilicet, quia oportet quemlibet adultum accedentem ad fidem credere quia Deus est et quia est remunerator omnium bonorum. Item oportet omnes alios articulos

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confession was never absent and may never be discounted. Yet it usually followed rather than preceded the liturgical practice that all took for granted—as in Innocent's reference to clerics who attended the altar without necessarily knowing exactly what happened there.

Fourth, after about the year 1000, Christendom, rooted in practice and profession and given shape by liturgical, ecclesiastical, and creedal structures, included every person in medieval Europe except the Jews. Certain religious observances were therefore expected of, and certain elements of religious culture were common to, all: baptism at birth and last rites at death to secure eternal salvation, rudimentary knowledge of the Apostles' Creed and Lord's Prayer, rest on Sunday and feast days (holy days) with attendance at mass, fasting at specified times, confession once a year after 1215 (usually Shrove Tuesday), communion at Easter, the payment of various fees and tithes at specified times, and alms for the needy (partly as a penitential exercise). Whatever their level of indifference, superstition, or immorality, every European grounded his or her religious life in this basic cultural structure.[92] But, since baptism now included all people, real differences, even "dualities," in expectations arose—and not just those between a literate clerical and a customary popular culture.

Medieval Christian texts refer constantly to the need for "conversion" and for "winning souls" (*lucrum animarum*). Between the eighth and twelfth centuries these expressions usually meant entry into a monastic order, an option taken by many "secular" clerics and noble laymen late in life but increasingly chosen by members of all social classes, for example, by the "lay brothers" (*conversi*). "Religious" referred most commonly therefore, as it still does in many languages today, to the professed and professionally religious.[93] After the thirteenth century, "winning

fidei credere implicite, idest credere verum esse quicquid credit ecclesia catholica. Hanc autem mensuram extendere debent praelati ecclesiarum quicumque curam animarum habent, quia debent scire articulos fidei explicite et distincte. . . . Non negamus tamen papa et alii superiores prelati possint dispensative ex certis causis minus literatos et minus industriosos in temporalibus sustinere. . . . De clericis autem inferioribus satis est—si essent pauperes qui non possent intendere studio, qui non habent magistros vel expensas, quia oportet eis propriis manibus quaerere victum—quod eis sufficiat scire sicut simplicibus laicis et aliquantulum plus, sicut de sacramento altaris; quia oportet eos credere quod in sacramento altaris conficitur verum corpus Christi, et hoc ideo quia circa illud quotidie et continue versantur plus quam laici." Innocentius IV, *In quinque libros decretalium Commentaria* (Venice, 1610), 2.

[92] Delaruelle culled the following from a fourteenth-century inquisition document: "Ut asserit, ut verus et fidelis christianus . . . frequentando ecclesias, divina officia audiendo, oblationesque faciendo, necnon annis singulis sua peccata confitendo et corrigendo, ac religiosas mendicantes in domo suo recipiendo"; *L'église*, 728.

[93] See Peter Biller, "Words and the Medieval Notion of 'Religion,'" *Journal of Ecclesiastical History*, 36 (1985): 351–69. Biller qualified the claim of an early modern historian that in the Middle Ages "religion" for the most part meant only the monastic life. See John Bossy, "Some Elementary Forms of Durkheim," *Past and Present*, 95 (1982): 4–5. Biller was right to point out that in the Middle Ages "religion" could also be understood as a "system of faith and morals" and that laymen could also be "religious." But it is important to note that in the passage from Cardinal Hostiensis' *Summa Aurea* on the *Decretals*, which Biller cited, people are "religious" because they live "stricter" and "holier" lives than others live: "Largo modo dicitur 'religiosus' qui in domo propria sancte et religiose vivit, licet non sit professus . . . dicitur talis 'religiosus' non ideo, quod astrictus sit alicui regulae certae, sed respectu vitae, quam arctiorem et sanctiorem ducit quam ceteres seculares qui omnino saeculariter, idest dissolute vivunt." *Summa Aurea* (Venice, 1574 edn., reprint edn., Turin, 1963), 1108; and Biller, "Words and the Medieval Notion of 'Religion,'" 358 n. 24. The language of a "stricter" life style was taken over from monasticism, and those other "dissolute worldly folk" were baptized Christians.

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souls" referred more often, especially among the mendicants, to bringing people to penance through preaching, though mendicant houses also had more and more *conversi*, confraternities, and other religious groups associated with them. The later Middle Ages produced a whole series of formal and informal groups comprised of laymen and secular clerics who wished to live more "religiously" by engaging in particular liturgical and penitential exercises.[94] That is to say, medieval folk themselves recognized real distance between the mass of baptized Christians and the "converted," between the "simple faithful" and the "religious," between those whose souls had and had not been "won."

The means to close the gap between "implicit" and "explicit" faith was instruction, but for men and women to go beyond the status of the merely baptized to the securely won required regular participation in the sacraments, especially confession and the eucharist, or entry into the formal religious life. Both instruction and the sacraments required an ordained priesthood. In the early Middle Ages, the intercession of the saints, the power of relics, and the prayers of monks seemed almost to overwhelm ordinary priestly functions, despite the importance of the viaticum and Christian burial. But from the twelfth century onward, the duality between ordained priests, at whatever social, intellectual, or even moral level, as dispensers of the life-bringing sacraments and a dependent laity obligated spiritually and materially to such intermediaries affected nearly every aspect of the religious culture.[95] Much of the talk about "lay piety" during the last twenty years has therefore been somewhat misleading. The whole inner dynamic was for the laity to acquire parts of the priestly sacred culture, whether it be abbreviated books of hours for noble women, set prayers for confraternities and tertiaries, chantry priests for guilds and patricians who could afford them, windows and burial sites in churches, or even stolen hosts and oils to use as charms. It is striking to note, for instance, that the Middle Dutch text with the largest number of extant manuscripts, more than eight hundred, is Geert Grote's translation of certain liturgical hours for the use of laymen and laywomen. Although there were certainly those, as Schmitt and Le Goff have argued, who sought access to the holy outside church-approved means, all the evidence suggests that the dynamic shaping religious culture from the eleventh century onward lay in the efforts of medieval folk to go beyond baptism, implicit faith, and days of obligation to acquire for themselves what ordained holy men, at whatever level, possessed as a privilege and maintained as a duty. The dynamic, that is, lay in movement from one set of expectations toward another, expectations that in

[94] For the religious and structural issues raised here, see Duane Osheim, "Conversion, *Conversi*, and the Christian Life in Late Medieval Tuscany," *Speculum*, 58 (1983): 368–90; Delaruelle, *L'église*, 666–94; Daniela Rando, "'Laicus religiosus' tra strutture civili ed ecclesiastiche: L'ospedale di Ognissanti in Treviso (sec. XIII)," *Studi Medievali*, 24 (1983): 617–56; and Kaspar Elm, "Die Bruderschaft vom Gemeinsamen Leben: Eine geschichtliche Lebensform zwischen Kloster und Welt, Mittelalter und Neuzeit," *Ons geestlijk erf*, 59 (1985): 470–96. The example that Hostiensis gave of a "religiosus" concerned a "hospital"; Biller, "Words and the Medieval Notion of 'Religion,'" n. 9.

[95] Among many studies on the importance of the sacerdotal-lay division, two recent works treat different aspects of its impact on religious culture. See Caroline W. Bynum, *Jesus as Mother* (Berkeley and Los Angeles, 1982), 3–8, 170–72; and Johannes Laudage, *Priesterbild und Reformpapsttum im 11. Jahrhundert* (Cologne, 1984).

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principle cut across social and intellectual lines. Such expectations might well have reinforced and coalesced with other social, economic, and cultural divisions. For example, monks and university men came predominantly from the upper social ranks, and simple country priests could enjoy sizable livings. The issue, however, is to recognize in a society of the baptized both the common elements underlying all of medieval religious culture and the distinctions in law and the social order as well as in religious expectations.

Fifth, the practice and profession that encompassed *Cristendom* was expounded in books and yet was learned chiefly by doing. Here the new school of historical interpretation has pinpointed one of the principal problems in interpreting medieval religious culture. Illiterate folk had somehow to appropriate religious teachings from books in another language—from the Bible, service books, law books, and so on—and to shape their religious lives accordingly. Using an anthropological perspective, historians of the new school have concluded that such a process was well nigh impossible or transpired mainly under duress. Kings forced their people to conform; priests compelled illiterate men and women to mumble certain words and perform certain rituals to guarantee their happiness in the hereafter; confessors tried to reshape the inner lives of reluctant penitents, and so on. To describe this process, these historians now use the term "acculturation," first coined by American anthropologists to describe what happens when an indigenous culture is colonized or subject to proselytizing by another, dominant culture.[96] A half-dozen papers (rather than a half-dozen sentences) would be required to review adequately this question of the interaction between an oral and a literate culture. Brian Stock has recently argued that "textual communities," as he nicely put it, formed during the eleventh and twelfth centuries, which reached out to include and to influence illiterate laymen high and low.[97] And, indeed, one reform movement after another in that period and later revolved around access to and implementation of a normative text. Yet Stock's argument, to work historically, requires him to "see through" well-known "clerical" texts and to discover oral communities behind them. This form of argument, much like Schmitt's and Le Goff's, is inevitably circular, for the state of those oral communities can only be inferred from texts that were supposedly the product of another culture. A significant expansion of textual communities (the plural is important) may have occurred in the twelfth century, but the medieval Christian community was, like the Jewish and Muslim communities to which it can be compared on this point, by its very nature a textual community.

Medieval Christians were notoriously slow in their development of a widespread literate religious culture. And yet every baptized person had some vague notion that the mysteries of his religion were contained in books, and more than a few

[96] For an excellent survey of the issues, see *Faire Croire*, 2–5. Also see Elikia M'Bokolo, "Acculturation," in *La nouvelle histoire*, ed. J. Le Goff *et al.* (Paris, 1978), 21–24. For a more critical view, see Jean Wirth, "Against the Acculturation Thesis," in von Greyerz, *Religion and Society in Early Modern Europe*, 66–78.

[97] For an introduction to this complicated subject, with helpful references, see, especially, the opening and closing chapters of Brian Stock's *The Implications of Literacy: Written Language and Models of Interpretation in the Eleventh and Twelfth Centuries* (Princeton, N.J., 1983).

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must have harbored some natural sense or curiosity about what the books said. Moreover, despite undeniable and continuing complaints in medieval visitation records about inadequate or missing liturgical books and the like, newer studies seem to indicate that by the Carolingian era those books were fairly well distributed, even at the lowest level of society.[98] The real interpretive issue must therefore turn on the ways and means found to transmit the mysteries of those religious texts and the people's reception or understanding of what was transmitted. The methods employed to teach religious ideas and practices included, especially, materials now studied by art and literary historians: mystery plays developing out of the mass, wall paintings as "books for the illiterate," vernacular sermons, saints' lives in epic form, and so on. A difficult area, this, and one in which historians often must work from hints rather than solid sources. For example, if Waldensians made a point of averting their eyes from the "images" on entering a church, Catholic folk, presumably, naturally looked to wall paintings and stained glass windows for the mysteries of their faith. If vernacular literature cannot be understood properly apart from its use of Scripture and figurative interpretations, as Friederich Ohly and D. W. Robertson have contended, clearly at least some knowledge of Scripture had made its way into lay culture.[99] And, if sculpted cathedral fronts really bore antiheretical messages, as some art historians argue, churchmen assumed evidently that laymen could understand them. There are, in addition, all those treatises—many in the vernacular—on the commandments, the creeds, the vices, and the virtues, which may well prove the most crucial sources through which to explore this question.[100] That the great majority of medieval folk were cut off from direct access to the written norms of a Christian culture is undeniable. But the real question is the degree to which their rituals, art, literature, and cosmology had nonetheless been shaped or influenced by those norms, that is to say, the degree to which Christian culture had over time become their oral culture. That concerned Christians were inevitably oriented toward the texts must also be taken seriously. Think of Waldo, Francis, and any number of others attempting to learn and implement the gospel. The process of increasing Christianization was not some brutal "acculturation" but rather an impetus built into a religious community based on books.

All this is difficult enough, but it is even more difficult to assess how medieval folk received and understood all that church practices and texts prescribed. The end product was inevitably some synthesis between the old and new cultures under the aegis of Christendom—my sixth point. Cultures do change, even oral and

[98] See, for instance, Carl Hammer, Jr., "Country Churches, Clerical Inventories, and the Carolingian Renaissance in Bavaria," *Church History*, 49 (1980): 5–17.

[99] Part of a fourteenth-century inquisitorial record from Piedmont reads as follows: "Nos bene vadimus ad ecclesiam et dicimus illud quod videtur nobis sed nos noluimus adorare picturas." "Quando erat in ecclesia, non debebat aspicere ymagines nec orare, sed solum habere cor Deo celli," as quoted by G. G. Merlo, *Eretici e inquisitori nella società piemontese del trecento* (Turin, 1977), 24 n. 27, and 25 n. 32. Friederich Ohly, *Schriften zur mittelalterlichen Bedeutungsforschung* (Darmstadt, 1977); and D. W. Robertson, *Essays in Medieval Culture* (Princeton, N.J., 1980).

[100] For one example, newly edited by I. Sullens, see Robert Mannyng of Brunne, *Handlyng Synne* (Binghamton, 1983), a Middle-English translation of an Anglo-French *Manuel des Pechiez*.

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customary ones, and at some undeterminable point most people in Europe came to consider themselves "Christian." This transition, from one customary religious culture to another, can only have been very slow with much retained from the old and absorbed from the new. But it is absurd to draw an absolute social and intellectual line between the "bookish" and the "customary" cultures. Clerics, after all, also came from among the folk and continued to work with them in villages, towns, and courts; villagers thought of the church, especially the nave, as "theirs"; great families often considered abbeys or bishoprics, including Rome's, as "theirs"; and even university men dealt routinely with bakers and innkeepers, magistrates and courtiers. A recent study of Thomas Aquinas, for instance, indicates that he understood a great deal of popular religious culture.[101]

Certainly, as Schmitt, Le Goff, and others have demonstrated, Christian materials could be—and were—appropriated and employed within mental frameworks still essentially of another religious sort. Christian motifs often functioned in non-Christian ways, and this must be taken seriously. But here two important qualifications must be made. First, while non-Christian attitudes often governed Christian usages—say, a "magical" view of the sacraments—practices of non-Christian origin early became endowed with Christian meaning. To retain the old name for the first day of the week (as in Germanic languages), though an interesting mark of the uneven process of conversion, was not after a while consciously to honor the sun rather than the Lord (as in the Romance languages). To cast a host into a raging fire, whatever the non-Christian roots of this practice, was to deploy the power believed inherent in the Body of Christ. Even to call on the powers ascribed to saints and relics was not to intercede with some non-Christian deity but with the person described in the legends read out on the appointed feast days and at the shrines. All this is not to deny the maintenance of much in medieval religious culture that was non-Christian in practice and belief, but it is to qualify the exaggerated claims made on the basis of studies into selected bits of medieval folklore. The synthesis was total, from the Christian as well as the non-Christian side, and the cultural dynamic lay with Christianization, recurring efforts to purify, domesticate, and rationalize.

Influence cut both ways. All the emphasis of late has fallen on the imposition of a new religious culture from on high by a clerical elite, its brutal disregard for the indigenous religious culture, and its failure to gain much ground (this slightly contradictory) until the seventeenth century, and there is certainly truth in such observations, however much they must be qualified by particular times, places, and instances. Written clerical culture, however, also absorbed a great deal from popular religious culture, as demonstrated plainly and most obviously in the various reform movements that recalled people from a "compromised" religious life style to one following more strictly the prescriptions of a given text (biblical, monastic, legal, or whatever).

[101] B. Lacroix and A. M. Landry, "Quelques thèmes de la religion populaire chez le théolgien Thomas d'Aquin,", in *La culture populaire*, 165–79.

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University-trained intellectuals frequently took as their point of departure the practice of Christianity, not just an internal analysis of its texts. This is overwhelmingly obvious in Gratian's *Decretum*, the university textbook for canon law, but the same holds true in theology. Hugh of St. Victor's summary gathering of texts "on the mysteries [sacraments] of the Christian faith" seeks precisely, especially in its second part on redemption, to explain practice, various acts of worship, grades of churchmen, signification of vestments, and so on. Even Peter Lombard's theological textbook, especially in Book IV on the "signs" the Christian community was to "use" (in Augustinian language from his *On Christian Learning*), presupposed practice. When schoolmen discussed at length, for instance, the validity and effectiveness of a baptism performed with incorrect, ungrammatical, or mixed-up words (and a host of similar questions),[102] they took up issues deeply rooted in practice, not just verbal gymnastics, and by making distinctions or stressing "intentionality" sought to move beyond the anxieties arising from what might be called a "magical" mentality. But in so doing they also gave new shape and form to many Christian teachings. Indeed, purgatory seems to have been one such notion,[103] and, ironically, Le Goff's own argument hinges on the point at which the Latin word made its appearance in clerical texts. The cult of the saints and of relics is another instance where popular practice outran theological articulation and ecclesiastical control; both were forced to catch up later. So, too, indulgences, accompanied by vague notions of "forgiveness" or "entry into heaven," were sought and dispensed long before the theological explanations or ecclesiastical manipulations of the "elite" could rationalize and exploit them. Admittedly, questions and texts on the how, why, and what of practice moved things ever further toward intellectual abstraction, especially after the universities reached full maturity as autonomous corporations in the course of the thirteenth century. But, to come full circle, the very gathering and ordering of the *exempla* in written documents reveals real concern with reaching, assessing, and encouraging, as well as repressing, popular religiosity. Scholars must take full account of both the syncretism and the recurrent Christianizing dynamic.

ADHERENCE TO THE PRACTICES, STRUCTURES, and obligations of Christendom could well be imposed, but Christianity could endure and shape religious culture only if it sprang from internal assent, however socially conditioned. No acculturation theory will adequately account for the people's concern to acquire and to preserve their Christendom. But neither can historians describe or measure those convictions in any detail, except when they are recorded in biographical or autobiographical accounts, rarely written by the great mass of ordinary medieval folk, or when they generated "conversions" and the accompanying changes in life style. Historians of medieval Christian culture will always be dependent far more than their early modern colleagues on the texts and sophisticated writings produced by

[102] Peter the Chanter *Summa de sacramentis et animae consiliis* 1.26–32.

[103] Gurevich wrote, "Therefore I tend to see both cultural traditions, scholarly and popular, at work together in the history of purgatory's 'birth'; the initiative belonged to the latter, and thinkers and theoreticians had to follow it." "Popular and Scholarly Medieval Cultural Traditions," 88.

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literate clerics, and they must concede the realities of a gap between common prescriptions for all Europeans and the great diversity of local practice, as well as between public conformity and inner conviction. But, before despairing or treating medieval religious life as comprised of two cultures, they should also recognize and explore all the many elements, from above and below, that were shared. Our image of medieval Christian culture, embracing both commonality and real distinctions, must necessarily be complex, partly because of the complexities of Judeo-Christian religion itself, partly because of the variety of traditions that went into its successive medieval constructs, and not least because of the propensity of medieval religious culture to turn repeatedly on itself in critique and reform. This last element received support as well from Scripture where the prophets and Jesus himself are shown severely criticizing the inherited religious culture of the chosen people; in medieval religious culture a population of baptized Christians constantly needed correction.

Then, too, for all the broad compass of medieval Christian culture, the "world" was still very much there, taken for granted in the texts and yet all the more difficult to discern because nearly everyone was necessarily a part both of the "world" and of "Christian culture." Insight into this duality may be gained from a late twelfth-century memoir. In one of the more personal passages of his *Courtier's Trifles*, Walter Map, an English cleric and also a courtier in the time of Henry II, described the terrible vices and vanities court life imposed on people and then urged his readers to maintain through it all a hidden devotion to the Trinity (in occulto colatur Trinitas). Elsewhere he spoke both of a holy hermit in the Welsh marchlands whose prayers, Map believed, had kept the hermit safe during a storm on the channel and of *succubi* (folkloric creatures) who plagued folks in that part of the world.[104] Here are all the elements present in medieval religious culture: worldliness and devotion, prayer and superstition. But the inner dynamic of it all, I would submit, sprang from a commitment to Christendom.

In sum, to study the Christianization of medieval religious culture, historians must take seriously medieval peoples' frequent use of that term in both Latin and the vernacular languages to describe their own loyal ties and civilization; the ritual, ecclesiastical, and legal mechanisms put in place to plant, foster, and sustain that religious culture at every social level; the dynamic inherent in acting on religious belief; the differing religious obligations and expectations established for a population of baptized Christians; the means and media used to communicate the teachings of written texts to an illiterate populace; and finally the inevitable synthesis of old and new in the resultant religious culture. Recent study of popular religious culture has forced historians to focus on its distinctive and sometimes non-Christian character and function. But any approach that denies the reality of Christianization as crucial to the formation and flowering of medieval religious culture will miss wholly its inner dynamic.

[104] Walter Map *De nugis curialium* D. 4.13, 2.2, 12.

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Reflections on the Medieval German Nobility

JOHN B. FREED

AS TIMOTHY REUTER OBSERVED, the "nobility has been one of the main concerns of medieval historians since the second world war," but "surprisingly little European scholarship on the subject is generally known in the English-speaking world."[1] Except for Reuter's own translations of a number of important articles, only Georges Duby's studies have been readily accessible to English and American students.[2] German scholars have concentrated on the origins of the nobility, the structural transformation of the aristocratic kindred, and the rise of the ministerials. German medievalists began to investigate these topics during the Nazi period,[3] but two postwar political developments have stimulated German interest in the nobility. The movement for a united Europe that has invoked Charlemagne as its patron saint—the headquarters of the Common Market are located in the Charlemagne Building in Brussels[4]—has focused attention on the common Frankish heritage of France and Germany and the nobility's role in the creation

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[1] Reuter, ed. and trans., *The Medieval Nobility: Studies on the Ruling Classes of France and Germany from the Sixth to the Twelfth Century*, Europe in the Middle Ages: Selected Studies, vol. 14 (Amsterdam, 1979), 1.

[2] Duby, *The Chivalrous Society*, trans. Cynthia Postan (Berkeley and Los Angeles, 1977), *Medieval Marriage: Two Models from Twelfth-Century France*, trans. Elborg Forster (Baltimore, Md., 1978), and *The Three Orders: Feudal Society Imagined*, trans. Arthur Goldhammer (Chicago, 1980).

[3] The aristocratic kindred was composed of members who could claim a common relationship, through either their father or their mother, with a magnate; its members did not necessarily share a common ancestor but were simply related to a specific individual. The ministerials were individuals of servile legal status, but of aristocratic life style, who formed in the twelfth century an estate, unique to the medieval kingdom of Germany. Hans K. Schulze has traced the postwar German interest in the medieval nobility to an article published by Heinrich Dannenbauer in 1941. See Schulze, "Rodungsfreiheit und Königsfreiheit: Zu Genesis und Kritik neuerer verfassungsgeschichtlicher Theorien," *Historische Zeitschrift*, 219 (1974): 530–31, and "Reichsaristokratie, Stammesadel und fränkische Freiheit: Neuere Forschungen zur frühmittelalterlichen Sozialgeschichte," *ibid.*, 227 (1978): 353. For Dannenbauer's article, see "Adel, Burg und Herrschaft bei den Germanen: Grundlagen der deutschen Verfassungsentwicklung," *Historisches Jahrbuch*, 61 (1941): 1–50. It was reprinted with the original pagination; Hellmut Kämpf, ed., *Herrschaft und Staat im Mittelalter*, Wege der Forschung, vol. 2 (Darmstadt, 1956).

[4] *New York Times*, March 20, 1984, p. 6.

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and dissolution of the Carolingian empire.[5] Second, the birth of a genuine Austrian nationalism during the Second World War and the establishment of the German Federal Republic have made *Landesgeschichte*—ultimately, the study of the principalities created by the medieval princes and thus long suspect as a disguised form of German particularism—academically respectable.[6]

A reluctance to read technical German prose and a general attitude of bitterness toward Germany after two world wars explain why most English-speaking scholars have ignored German scholarship on the medieval nobility, but those historians who have ventured into this area have probably been repulsed for two other reasons as well. First, German scholarship remains highly politicized because the study of the Middle Ages is not a politically neutral topic in the German-speaking world and because German social historians have used the study of social history to seek explanations for German national disunity. Second, many of the underlying premises of German scholarship on the medieval period—for example, the linkage of freedom with service—are repugnant to English-speaking scholars steeped in the Western tradition of natural rights. An examination of the ideas of the three men who have dominated postwar German studies of the medieval nobility reveals that many of the premises underlying their work did in fact originate or become popular during the Third Reich. While this does not necessarily invalidate their findings, it does suggest that their work, valuable and important as it has been, requires further scrutiny. The scholars in question are: Gerd Tellenbach, who concentrated on the Carolingian imperial aristocracy, Karl Schmid, who investigated the change in family structure during the Saxon and Salian eras, and Karl Bosl, who studied the role of the ministerials in the Hohenstaufen revival.

TELLENBACH FIRST PRESENTED HIS VIEWS about the Carolingian imperial aristocracy (*Reichsaristokratie*) in 1939 in a book on the so-called newer stem duchies.[7] This aristocracy was composed primarily of Frankish sips, or clans, who came from the heartland of the empire, the region between the Meuse and the Moselle. These nobles held the chief offices, clerical and secular, in the empire and had extensive property holdings throughout Western Europe but were highly dependent on the

[5] See, for instance, Josef Fleckenstein, *Early Medieval Germany*, trans. Bernard S. Smith, Europe in the Middle Ages: Selected Studies, vol. 16 (Amsterdam, 1978), xi–xv, 75–86; and Karl Ferdinand Werner, *Structures politiques du monde franc (VI^e–XII^e siècles): Etudes sur les origines de la France et de l'Allemagne* (London, 1979), i–iii. Fleckenstein's book was published originally as *Grundlagen und Beginn der deutschen Geschichte*, Deutsche Geschichte, vol. 1 (Göttingen, 1976).

[6] Reuter pointed out that there has been little agreement about the purpose or methodology of *Landesgeschichte*; Reuter, "A New History of Medieval Germany," *History*, 66 (1981): 442. Pankraz Fried has collected a number of theoretical articles on the topic. See Fried, *Probleme und Methoden der Landesgeschichte*, Wege der Forschung, vol. 492 (Darmstadt, 1978).

[7] Tellenbach, *Königtum und Stämme in der Werdezeit des Deutschen Reiches*, Quellen und Studien zur Verfassungsgeschichte des Deutschen Reiches in Mittelalter und Neuzeit, vol. 7, pt. 4 (Weimar, 1939), esp. 41–69. The stem duchies were tribal units that the Merovingians incorporated into the Frankish kingdom but that retained considerable autonomy under the leadership of dukes appointed by the king. The Carolingians abolished the stem duchies, but the great magnates, acting largely on their own authority and drawing on this tribal legacy, created the duchies of Bavaria, Franconia, Saxony, and Swabia, the so-called newer stem duchies, around 900.

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ruler's favor. As the Carolingian empire disintegrated in the course of the ninth century, the sips were gradually provincialized, and their members became the creators of the kingdoms, principalities, and stem duchies that arose out of the ruins of Charlemagne's empire. In an article published in 1943, Telienbach extended his analysis to the twelfth century and traced the connection between the Carolingian imperial aristocrats and their descendants, the German princes who created the individual principalities.[8] Since the Second World War, Tellenbach and the other members of the Freiburg School (O. Baumhauer, Josef Fleckenstein, Eduard Hlawitschka, Karl Schmid, H. M. Schwarzmaier, R. Sprandel, Franz Vollmer, and Joachim Wollasch) have studied sips, like the Welfs, in more detail.[9]

It should be pointed out that Tellenbach refined his interpretation of the Carolingian imperial aristocracy during a debate with Martin Lintzel and Walter Schlesinger in 1941 and 1942 about the origins of the first German Reich. Tellenbach stressed the actions of the king, while his opponents emphasized the role of the folk led by the nobles. This is especially clear in the disputants' discussion of the deposition of Charles III and the accession of Arnulf of Carinthia in 887.[10] The choice of such a topic at the height of the Third Reich can hardly have been coincidental. Schlesinger defended himself in his preface to the 1964 edition of *Die Entstehung der Landesherrschaft* (1941) against the accusation that the subtitle of one of his chapters, "The Seignorial Structure of the Folk Order," had Nazi overtones,[11] and Tellenbach's description of how the dukes formed the politically unorganized stem into a duchy betrays the period of its composition. "The stem duchy did not arise out of the fragmented will of the leaderless [*führerlosen*] stem but rather out of the duke's determination to rule. The duke himself was the political organization of the hitherto unorganized leaderless [*führerlosen*] stem."[12] Although Lintzel was initially skeptical about describing a sip

[8] Tellenbach, "Vom karolingischen Reichsadel zum deutschen Reichsfürstenstand," in Theodor Mayer, ed., *Adel und Bauern im deutschen Staat des Mittelalters* (1943; reprint edn., Darmstadt, 1967), 22–73. This article was reprinted in Kämpf, *Herrschaft und Staat*, 191–242. Reuter translated the article into English; "From the Carolingian Nobility to the German Estate of the Princes," *The Medieval Nobility*, 203–42.

[9] Tellenbach, "Kritische Studien zur grossfränkischen und alemannischen Adelsgeschichte," *Zeitschrift für württembergische Landesgeschichte*, 15 (1956): 169–90; and Tellenbach, ed., *Studien und Vorarbeiten zur Geschichte des grossfränkischen und frühdeutschen Adels*, Forschungen zur oberrheinischen Landesgeschichte, vol. 4 (Freiburg, 1957). I derived the list of Tellenbach's associates from his "Kritische Studien" (p. 169, headnote) and from the contributors in *Studien und Vorarbeiten*. On the Welfs, see Tellenbach, "Über die ältesten Welfen im West- und Ostfrankenreich," in Tellenbach, *Studien und Vorarbeiten*, 335–40; and Fleckenstein, "Über die Herkunft der Welfen und ihre Anfänge in Süddeutschland," *ibid.*, 71–136.

[10] Schlesinger, "Kaiser Arnulf und die Entstehung des deutschen Staates und Volkes," *Historische Zeitschrift*, 163 (1941): 457–70; Tellenbach, "Zur Geschichte Kaiser Arnulfs," *ibid.*, 165 (1942): 229–45; and Lintzel, "Zur Stellung der ostfränkischen Aristokratie beim Sturz Karls III. und der Entstehung der Stammesherzogtümer," *ibid.*, 166 (1942): 457–72. These articles have been reprinted with the original pagination. See Kämpf, ed., *Die Entstehung des Deutschen Reiches (Deutschland um 900)*, Wege der Forschung, vol. 1 (Darmstadt, 1956). In addition, see Tellenbach, *Königtum und Stämme*; and Schlesinger, *Die Entstehung der Landesherrschaft*, Sächsische Forschungen zur Geschichte, vol. 1 (1941; reprint edn., Darmstadt, 1964), 130–32. On this debate, see Werner, *Das NS-Geschichtsbild und die deutsche Geschichtswissenschaft* (Stuttgart, 1967), 51–52.

[11] Schlesinger, *Entstehung der Landesherrschaft*, xiv.

[12] Tellenbach, *Königtum und Stämme*, 92. Geoffrey Barraclough popularized in the English-speaking world Tellenbach's thesis that the dukes created the duchies during the reign of Conrad I; Barraclough,

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like the Liudolfingians, the ancestors of the tenth-century Saxon kings of Germany, as simply a "creature of the king" and had misgivings about the meaning of *Reichsaristokratie*,[13] the term has become standard in postwar German historiography.[14]

Tellenbach's theory of an imperial aristocracy can be criticized on a number of grounds. The first, methodological. Although members of the Freiburg School have repeatedly cautioned historians about using distinctive names that appear frequently in the sources to fill in gaps in family trees before the eleventh century,[15] the temptation to arrange the few surviving pieces of the jigsaw puzzle into a complete picture has proved overwhelming, even for Tellenbach and his colleagues. For instance, in his discussion of Frankish aristocrats who served in Italy, Tellenbach wrote: "One must judge equally carefully about Duke Garamannus, about whom Pope Adrian I complained in 787/91 because he had seized properties of the church of Ravenna. We find several references in Alamannia to a Count Caramann between 797 and 820, yet another one in 834, and then between 838 and 850 to a witness Caramann [Craman]. Count Caramann was, according to the testimony of witnesses about St. Gall's property in Vilsingen, a relative of the Berthold sip. He is undoubtedly the Count Caramann who is named on several occasions in the Book of Confraternity of Reichenau. He could have been the same person as the Italian duke."[16] Although Tellenbach did not actually say that the Italian duke and the Alamannian count were the same person, his overall thesis about an international imperial aristocracy with interests on both sides of the Alps would have been better served if they were. How likely is it, however, that a duke about whom the pope complained in 787/91 would still have been alive in 834, let alone in 850?

The danger in equating people who had identical or similar names is illustrated by Heinz Dopsch's critical comments about previous studies of the Otakare, the twelfth-century margraves of Styria, whose ancestry has been traced by other scholars to the eighth century. Dopsch pointed out that the Frankish duke

The Origins of Modern Germany (2d edn., New York, 1947), 21–23. On this point, see Tellenbach, *Königtum und Stämme*, 70–100. The origins of the so-called newer stem duchies has been one of the most controversial issues in postwar German historiography on the medieval period. For additional information, see John B. Freed, "Germany, Stem Duchies," *The Dictionary of the Middle Ages* (New York, 1985), vol. 5, pp. 505–11.

[13] Lintzel, "Stellung der ostfränkischen Aristokratie," 162–64.

[14] See, for instance, Bruno Gebhardt, *Handbuch der deutschen Geschichte*, ed. Herbert Grundmann (9th edn., Stuttgart, 1970), vol. 1, pp. 192–95, 719–21.

[15] Schmid, "Königtum, Adel und Klöster zwischen Bodensee und Schwarzwald (8.–12. Jahrhundert)," in Tellenbach, *Studien und Vorarbeiten*, 330–31, "Über die Struktur des Adels im früheren Mittelalter," *Jahrbuch für fränkische Landesforschung*, 19 (1959): 1–7, and "Zur Problematik von Familie, Sippe und Geschlecht, Haus und Dynastie beim mittelalterlichen Adel: Vorfragen zum Thema, 'Adel und Herrschaft im Mittelalter,'" *Zeitschrift für die Geschichte des Oberrheins*, 105 (1957): 1–16; and Tellenbach, "Kritische Studien," 169–70, 179–80. Schmid's articles "Über die Struktur des Adels" and "Zur Problematik von Familie" have been reprinted in Schmid, *Gebetsgedenken und adliges Selbstverständnis im Mittelalter: Ausgewählte Beiträge; Festgabe zu seinem sechzigsten Geburtstag* (Sigmaringen, 1983), 245–67, 183–244. Reuter translated "Über die Struktur des Adels." See "The Structure of the Nobility in the Earlier Middle Ages," *The Medieval Nobility*, 38–42.

[16] Tellenbach, "Der grossfränkische Adel und die Regierung Italiens in der Blütezeit des Karolingerreiches," in Tellenbach, *Studien und Vorarbeiten*, 65.

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Autchar, who fled with Carloman's widow to the court of Desiderius in 771 and who became the model of the epic hero Ogier le Danois, could not have been the same person as the Otakar who was at the moment of Autchar's flight to Italy a benefactor of Fulda, and it is not known whether either man was identical with the Otakar who founded the Bavarian monastery of Tegernsee around 762/65.[17] In this case we are dealing, as Dopsch has shown, with two, if not three, contemporary prominent men with the same name or variants thereof. Such evidence can be subjected to numerous permutations that are the scholarly equivalent of a magician pulling rabbits out of a hat. Tellenbach and his associates have created an extremely useful methodology for studying the early medieval nobility, but it must be used with great care.

In 1958, in a review of Tellenbach's studies, Bosl levied a more fundamental criticism at the Freiburg School: it was ignoring the Germanic *Uradel*, the stem nobility, in its work.[18] Since then Austrian and Bavarian scholars have studied how the imperial aristocracy and the stem nobility coalesced in southeastern Germany. Michael Mitterauer demonstrated that Frankish imperial aristocrats, a few of whom, such as Count Witagowo of the Carantanian mark, were actually of Gallo-Roman ancestry, intermarried with Bavarian and even Slavic nobles. For instance, the Sighardinger, partisans of Louis the German's son Carloman, came from the Rhineland to the southeastern Carolingian marks, where they married Bavarian noblewomen.[19] Their descendants played a prominent role in Austro-Bavarian affairs until the thirteenth century.[20] Friedrich Prinz and Wilhelm Störmer, Bosl's own students, have emphasized that the Bavarian nobility in the Freising area already had close ties to the Carolingians before Bavaria's final incorporation into the Frankish empire. Prinz even called the five noble kindreds (who were described in the eight-century *Lex Baiuvariorum* as being second in rank only to the ducal house) Charlemagne's Fifth Column in Agilulfingian Bavaria.[21] Ironically, the last Agilulfingian duke, Tassilo III, whose mother was the sister of Pepin the Short, was himself of Burgundian-Frankish ancestry.[22] The contrast

[17] Dopsch, "Die steirischen Otakare: Zu ihrer Herkunft und ihren dynastischen Verbindungen," in Gerhard Pferschy, ed., *Das Werden der Steiermark: Die Zeit der Traungauer*, Veröffentlichungen des steiermärkischen Landesarchives, vol. 10 (Graz, 1980), 78–83.

[18] Bosl, "Reichsaristokratie und Uradel," *Zeitschrift für bayerische Landesgeschichte*, 21 (1958): 138–45.

[19] Mitterauer, *Karolingische Markgrafen im Südosten: Fränkische Reichsaristokratie und bayerische Stammesadel im österreichischen Raum*, Archiv für österreichische Geschichte, vol. 123 (Vienna, 1963), 144–53, 212–27, and "Slawischer und bayrischer Adel am Ausgang der Karolingerzeit," *Carinthia I*, 150 (1960): 693–726.

[20] Dopsch, *Geschichte Salzburgs: Stadt und Land*, volume 1: *Vorgeschichte, Altertum, Mittelalter*, 3 pts. (Salzburg, 1981–84), pt. 1, p. 363.

[21] Störmer, *Adelsgruppen im früh- und hochmittelalterlichen Bayern*, Studien zur bayerischen Verfassungs- und Sozialgeschichte, vol. 4 (Munich, 1972), *Vorwort*, 3, 6; and Prinz, "Herzog und Adel im agilulfingischen Bayern: Herzogsgut und Konsensschenkungen vor 788," *Zeitschrift für bayerische Landesgeschichte*, 25 (1962): 283–311. Prinz's article has been reprinted with the original pagination; Bosl, ed., *Zur Geschichte der Bayern*, Wege der Forschung, vol. 60 (Darmstadt, 1965).

[22] Erich Zöllner, "Die Herkunft der Agilulfinger," *Mitteilungen des Instituts für österreichische Geschichtsforschung*, 59 (1951): 245–64; and Josef Siegwart, "Zur Frage des alemannischen Herzogsgutes um Zürich: Beitrag zur Genealogie des alemannisch-bayrischen Herzogshauses," *Schweizerische Zeitschrift für Geschichte*, 8 (1958): 145–92. Zöllner's article has been reprinted with the original pagination; Bosl, *Zur Geschichte der Bayern*. Siegwart's article has been reprinted; Wolfgang Müller, ed., *Zur Geschichte der Alemannen*, Wege der Forschung, vol. 100 (Darmstadt, 1975), esp. 237–49.

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between the Frankish imperial aristocracy and the Bavarian stem nobility ought not to be drawn too sharply.

Still, Bavaria was one of the last areas to be incorporated into the Carolingian empire, and it is thus not particularly surprising that Bavaria retained more of its Germanic character than the more western parts of Charlemagne's realm. As Léopold Genicot has pointed out, German scholars have always tended to believe in the survival of the *Uradel*, while French historians, such as Paul Guilhiermoz and Marc Bloch, have maintained that the Merovingians exterminated both the Gallo-Roman senatorial aristocracy and the Frankish stem nobility.[23] Karl Ferdinand Werner has argued, however, that both groups survived and merged in Merovingian Gaul and that the Carolingians depended on the nobility of Neustria and Burgundy as well as that of Austrasia in their rise to power.[24] Charlemagne's alliance with the western Bavarian nobles against Tassilo III, his cousin and rival prince, and the subsequent coalescing of the Frankish and Bavarian nobility to form the high nobility of medieval Bavaria may not have been exceptional after all. If Werner is correct, little remains of Tellenbach's Carolingian imperial aristocracy recruited from the area between the Meuse and the Moselle and dependent on the king's favor. In essence, Werner, like Lintzel and Schlesinger, has stressed the Carolingians' dependence on the nobility. The picture that emerges is one of extraordinary continuity in the composition of the upper stratum of the nobility between the Merovingian and Hohenstaufen periods. The princes of the twelfth century were the descendants of nobles who had assisted the Carolingians in their rise to power and in the governance of the Frankish empire and who had been the chief beneficiaries of its dissolution.

Constance B. Bouchard has challenged this picture in the case of the French nobility. She argued that demonstrations of biological continuity between the nobility of the Carolingian period and the nobility of the twelfth century—for example, Werner's work on the Touraine and Duby's revised study of the Mâconnais—do not disprove the theory that the composition of the noble elite changed between the ninth and twelfth centuries because new men were able to marry into established lineages. Essentially, Werner and Duby have merely proved that upstarts became the sons-in-law of "noble fathers with a large number of eligible daughters." This explains in Bouchard's opinion why the size of the French noble elite expanded dramatically between the ninth and twelfth centuries and why noble lineages like the Angevins, whose ancestry can be traced to the

[23] Genicot, "Les recherches relatives à la noblesse médiévale," *Académie royale de Belgique: Bulletin de la classe des lettres et des sciences morales et politiques*, 5th ser., 61 (1975): 59–63. This has been reprinted with the original pagination; Genicot, *La noblesse dans l'Occident médiéval* (London, 1982). See Guilhiermoz, *Essai sur l'origine de la noblesse en France au moyen âge* (1902; reprint edn., New York, 1960), 1–4; and Bloch, *Feudal Society*, 2 vols. (Chicago, 1964), 2: 283–84.

[24] Werner, "Bedeutende Adelsfamilien im Reich Karls des Grossen: Ein personengeschichtlicher Beitrag zum Verhältnis von Königtum und Adel im frühen Mittelalter," in H. Beumann, ed., *Karl der Grosse, Lebenswerk und Nachleben*, volume 1: *Persönlichkeit und Geschichte* (Düsseldorf, 1965): 83–142. This has been translated by Reuter; "Important Noble Families in the Kingdom of Charlemagne—A Prosopographical Study of the Relationship between King and Nobility in the Early Middle Ages," *The Medieval Nobility*, 137–202.

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Carolingian period, were viewed in the twelfth century as the descendants of upstarts.[25]

While Germany in the Ottonian and early Salian periods was considerably more stable than tenth-century France, Bouchard's thesis may also be partially applicable to Germany. Many German nobles improved their positions by fortuitous marriages. For instance, the Spanheimer, the dukes of Carinthia from 1122 to 1269, were the descendants of Siegfried of Spanheim, who married Richgard, the daughter of Count Engelbert, whose inheritance included extensive properties in Carinthia and Bavaria. An entry in the codex of traditions of the Benedictine monastery of St. Paul's in the Lavant Valley described their son Count Engelbert I, the father of the first duke, as "Count Engelbert through his father Siegfried a Frank, but through his mother Richgard first among the magnates of Carinthia."[26] There may even have been some real *novi homines* among the princes of the twelfth century. Lutz Fenske has demonstrated that Louis I, who became the landgrave of Thuringia in 1131, was the grandson of a minor nobleman.[27] Finally, studies of the continuity of the German noble elite have concentrated on the princely and comital dynasties, but continuity among these groups does not necessarily mean that the large number of minor noble lineages that are mentioned—say, in the codices of traditions of the various Bavarian churches in the twelfth century—were the descendants of the Carolingian imperial aristocracy or Bavarian stem nobility. For example, the first known members of the Felben and Walchen lineages, the two most important noble families in the Pinzgau in the Austrian province of Salzburg, appeared only in the first half of the twelfth century.[28] It would be presumptuous to suppose that, if only the extant documentation were better, we could trace the Felbens' and Walchens' ancestry to the ninth century. In short, although Tellenbach and the Freiburg School have enormously increased our understanding of the noble elite of the Carolingian empire and its role in the formation of the successor kingdoms and principalities that emerged from the ruins of Charlemagne's realm, the origins of minor noble lineages like the Felbens and Walchens remain largely unknown. An investigation

[25] Bouchard, "The Origins of the French Nobility: A Reassessment," *AHR*, 86 (1981): 528; Werner, "Untersuchungen zur Frühzeit des französischen Fürstentums (9.–10. Jahrhunderts)," *Die Welt als Geschichte*, 18 (1958): 256–89; 19 (1959): 146–93; 20 (1960): 87–119; and Duby, "Lignage, noblesse et chevalerie au XII^{e} siècle dans la région mâconnaise: Une révision," *Annales, économies, sociétés, civilisation*, 27 (1972): 803–23. Duby's article has been translated by Postan; "Lineage, Nobility, and Knighthood: The Mâconnais in the Twelfth-Century—A Revision," *The Chivalrous Society*, 59–80.

[26] *Monumenta historica ducatus Carinthiae*, ed. August von Jaksch (1904; reprint edn., Klagenfurt, 1978): vol. 3, pp. 188–90, no. 488. On the origins of the Spanheimer, see Dopsch, "Die Grafen von Lebenau," *Das Salzfass: Heimatkundliche Zeitschrift des Historischen Vereins Rupertiwinkel*, new ser., vol. 4, pt. 2 (1970): 33–40. Two other men who made fortuitous marriages were Margrave Azzo of Este, who married the sister of the last Welf in the male line and whose son, Welf IV, continued the "Welf" lineage, and Frederick, the nephew of Archbishop Frederick I of Salzburg (958–91), who married a Swabian heiress and became the ancestor of the Hohenstaufen. On the latter, see Hansmartin Decker-Hauff, "Das staufische Haus," *Die Zeit der Staufer: Geschichte-Kunst-Kultur* (Stuttgart, 1977), 339–43.

[27] Fenske, *Adelsopposition und kirchliche Reformbewegung im östlichen Sachsen: Entstehung und Wirkung des sächsischen Widerstandes gegen das salische Königtum während des Investiturstreits*, Veröffentlichungen des Max-Planck-Instituts für Geschichte, vol. 47 (Göttingen, 1977), 241–46. Another example is Wiprecht of Groitzsch, who was enfeoffed with Meissen in 1123. *Ibid.*, 255–64.

[28] Dopsch, *Geschichte Salzburgs*, pt. 1, pp. 393–96.

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of their origins would undoubtedly alter the present view of the almost caste-like structure of the German nobility between the ninth and twelfth centuries and the perception that the Carolingian order survived in Germany until the Investiture Contest.

KARL SCHMID HAS STUDIED THE CHANGE in structure of the aristocratic kindred, and the change in the nobility's self-consciousness, during the Saxon and Salian periods. In "Welfisches Selbstverständnis," perhaps his most brilliant article, he analyzed how the twelfth-century Welfs, who were eager to learn more about their ancestry, could have overlooked their famous ninth-century West Frankish relatives, including the Welf kings of Burgundy, or forgotten that Judith and Hemma, the daughters of the first known Welf, had been married to two Carolingian monarchs, Louis the Pious and Louis the German. The Welfs remembered instead Eticho and his son Henry with the Golden Plough, who established the family's lordship in Swabia around 900.[29]

Bloch thought such ignorance indicated that the nobility, which he defined as a hereditary, legally privileged social class,[30] did not exist before the twelfth century. But Schmid attributed both the nobility's interest in and ignorance about its ancestry to a fundamental change in noble self-consciousness and family structure that occurred around 1000. During the Carolingian and early Ottonian periods, the nobility was organized, according to Schmid, in amorphous sips composed of men and women, both important and insignificant, who could claim kinship, however remote, with a powerful magnate. The kinship system was not patriarchal; enates—that is, maternal relatives—could be more important than agnates—kin related patrilineally—if the enates were more distinguished in their ancestry, wealth, and social position. Special emphasis was placed on relatives of royal blood because proximity to the royal house raised a sip's status and chances for advancement. As a member of the Freiburg School, Schmid emphasized that the gain or loss of royal favor was a major factor in the formation and dissolution of sips. Since individuals had only one name and since such names could be inherited from maternal as well as paternal ancestors, the attempts by modern scholars and genealogists to identify and label specific sips on the basis of distinctive names—for example, the Sighardinger—are, Schmid concluded, simply an anachronistic effort to impose a patrilineal family structure on people who did not think in such terms.

[29] Schmid, "Welfisches Selbstverständnis," in Fleckenstein and Schmid, eds., *Adel und Kirche: Gerd Tellenbach zum 65. Geburtstag* (Freiburg, 1968), 389–416. This article has been reprinted; Schmid, *Gebetsgedenken und adliges Selbstverständnis*, 424–53. There are other examples of such selective memories. In the 1160s Count Siboto IV of Falkenstein recalled that his mother's grandfather had been named Gerold, when he had been in fact the first Siboto. Freed, *The Counts of Falkenstein: Noble Self-Consciousness in Twelfth-Century Germany*, Transactions of the American Philosophical Society, vol. 74, pt. 6 (Philadelphia, 1984), 33–35. Bishop Otto of Freising thought that the Babenbergs, his lineage, were the descendants of the Frankish count Adalbert, who had been executed in 906 during the feud between the Conradins and the Old Babenbergs, but modern scholarship suggests that the Babenbergs belonged to the sip of the Liutpoldingians, the tenth-century dukes of Bavaria. Karl Lechner, *Die Babenberger: Markgrafen und Herzoge von Österreich*, Veröffentlichungen des Instituts für österreichische Geschichtsforschung, vol. 23 (Vienna, 1976), 39–45.

[30] Bloch, *Feudal Society*, 2: 283–85.

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As office holding became hereditary in the tenth and eleventh centuries, greater emphasis was placed on agnate descent as evident in the increasing use of first names inherited in the male line in successive or alternate generations. The royal family, the only real dynasty in early medieval Europe, offered a model for the rest of the nobility. The holders of hereditary offices were able to consolidate their possessions in the area where they exercised power, and by the middle of the eleventh century they were beginning to be identified with the castles that served as the centers of their lordships. The foundation of a dynastic monastery, which served as the family burial place and which preserved the lineage's traditions, stimulated and strengthened this sense of family identity. Initially, one man often employed a number of different cognomina to identify himself, but, as the nobility's sense of belonging to specific lineages intensified in the twelfth and thirteenth centuries, toponyms became fixed and turned into surnames. The great magnates adopted this practice first, but the custom gradually spread to lower social orders. The adoption of such hereditary surnames completed the transformation of the sips into patrilineal dynasties. The nobles of the twelfth century had such difficulties in reconstructing their ancestry because the kinship structure and self-perception of the nobility changed, not because the nobility was nonexistent before 1100.[31]

Schmid's work has been extremely influential, both inside and outside Germany. For instance, in examining the Capetian custom of making the king's eldest son a coruler in his father's lifetime, Andrew W. Lewis used Schmid's analysis of Henry I's designation of Otto I as his heir in 929 as a model. Indeed, Lewis retained the German words *Sippe* and *Geschlecht* in his own analysis.[32] More important, Duby, who has acknowledged his debt to Schmid in several places, employed Schmid's theory about the formation of patrilineal lineages to reinterpret his findings about the origins of the nobility of the Mâconnais.[33]

[31] Schmid, "Adel und Reform in Schwaben," in Fleckenstein, ed., *Investiturstreit und Reichsverfassung*, Vorträge und Forschungen, vol. 17 (Sigmaringen, 1973), 295–319, "Königtum, Adel und Klöster" and "Die Mönchsgemeinschaft von Fulda als sozialgeschichtliches Problem," *Frühmittelalterliche Studien: Jahrbuch des Instituts für Frühmittelalterforschung der Universität Münster* (hereafter, *FS*), 4 (1970): 173–200, "Neue Quellen zum Verständnis des Adels im 10. Jahrhundert," *Zeitschrift für die Geschichte des Oberrheins*, 108 (1960): 185–232, "Religiöses und sippengebundenes Gemeinschaftsbewusstsein in frühmittelalterlichen Gedenkbucheinträgen," *Deutsches Archiv für Erforschung des Mittelalters*, 21 (1965): 18–81, "Über die Struktur des Adels," "Über das Verhältnis von Person und Gemeinschaft im früheren Mittelalter," *FS*, 1 (1967): 225–49, "Welfisches Selbstverständnis," "Zur Problematik von Familie, Sippe und Geschlecht," and Schmid and Joachim Wollasch, "Die Gemeinschaft der Lebenden und Verstorbenen in Zeugnissen des Mittelalters," *FS*, 1 (1967): 365–405. The following articles, cited here for the first time, have also been reprinted in Schmid's *Gebetsgedenken und adliges Selbstverständnis*: "Adel und Reform," "Religiöses und sippengebundenes Gemeinschaftsbewusstsein," and "Über das Verhältnis von Person und Gemeinschaft."

[32] Lewis, *Royal Succession in Capetian France: Studies on Familial Order and the State* (Cambridge, Mass., 1981), 1–43. Oddly enough, Lewis did not cite in his bibliography the two articles that are most relevant to his thesis: Schmid, "Neue Quellen," 186–203, and "Die Thronfolge Ottos des Grossen," *Zeitschrift der Savigny-Stiftung für Rechtsgeschichte, Germanistische Abteilung*, 81 (1964): 80–163. The section of "Neue Quellen" that is cited here has been reprinted with an addition; Eduard Hlawitschka, ed. *Königswahl und Thronfolge in ottonisch-frühdeutscher Zeit*, Wege der Forschung, vol. 178 (Darmstadt, 1971), 389–416. "Die Thronfolge Ottos des Grossen," has been reprinted in the same collection (pp. 417–508).

[33] Freed, *Counts of Falkenstein*, 3–5.

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Schmid's critics have focused in large part on details rather than on substantive issues. There has been some concern about Schmid's evidence. Karl Leyser questioned whether Schmid's use of the *Libri memoriales*, monastic books of remembrance, as his chief source might not have led him to overstate the sip consciousness of the early medieval nobility. There is a difference, Leyser suggested, between remembering distant kinsmen in prayer and bequeathing property to them.[34] Schmid's admission that many monastic foundations were destroyed before the eleventh century by relatives of the founders who felt cheated by the donors' beneficence bears out Leyser's criticism.[35] Similarly, historians have expressed doubts that the adoption of a toponymic surname indicates the development of a patrilineal family consciousness. Störmer contended that at least some of the magnates developed a sense of family identity before they consolidated their territorial lordships and built the castles that were the centers of their lordships and that provided their surnames. For instance, the Bavarian counts of Mödling named the castle of Mödling, which they built near Au in the 1120s, after a village in the Chiemgau, twenty kilometers away, where they owned property. In other words, they perceived themselves as the Mödlings before they built the castle. Störmer also argued that the Bavarian magnates were not the first to adopt surnames. They were well known in the area where they lived and exerted their power, and they required no further identification. It was the lesser nobles who first employed surnames in the first half of the eleventh century—a practice soon copied by the ministerials—while dynasties of comital rank adopted surnames only in the second third of the twelfth century.[36] My own study of the Austro-Bavarian counts of Falkenstein indicates that noble families changed their cognomina with bewildering frequency. The counts of Falkenstein employed seven different surnames in a span of 160 years, and one individual could use several different surnames, often simultaneously.[37]

Leyser maintained that the only way to show that a stable patrilineal family structure developed in Germany was to demonstrate, as Duby did in the case of the Mâconnais, that the inheritance rights of other heirs were limited in favor of the eldest son.[38] This happened to the daughters and younger sons of the Falkenstein counts, but, in general, Leyser's criticism is well taken. German scholars have not investigated how changes in self-perception among the nobility affected the rights of daughters and younger sons and how family strategies that restricted their inheritance rights contributed to the rapid decimation of the

[34] Leyser, "The German Aristocracy from the Ninth to the Early Twelfth Century: A Historical and Cultural Sketch," *Past and Present*, 41 (1968): 32–34. This article has been reprinted; Leyser, *Medieval Germany and Its Neighbors, 900–1250* (London, 1982), 168–70. For Leyser's comments, see *Rule and Conflict in an Early Medieval Society: Ottonian Saxony* (London, 1979), 50. Genicot repeated Leyser's criticisms; "Recherches relatives à la noblesse médiévale," 64.

[35] Schmid, "Adel und Reform," 304–06.

[36] Störmer, "Adel und Ministerialität im Spiegel der bayerischen Namengebung (bis zum 13. Jahrhundert): Ein Beitrag zum Selbstverständnis der Führungsschichten," *Deutsches Archiv für Erforschung des Mittelalters*, 33 (1977): 119–31. On the Mödlings, see Günther Flohrschütz, "Die Vögte von Mödling und ihr Gefolge," *Zeitschrift für bayerische Landesgeschichte*, 38 (1975), 3–143.

[37] Freed, *Counts of Falkenstein*, 52–57.

[38] Leyser, "German Aristocracy," 50–51.

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nobility during the High Middle Ages.[39] For example, every noble family in the principality of Salzburg had either died out or entered the archiepiscopal ministerialage by 1250.[40]

Another issue in German historiography concerns whether the importance of paternal and maternal relatives differed in various time periods. Prinz and Störmer, employing evidence from Bavarian legal and ecclesiastical records, have argued that Schmid overemphasized the importance of enate versus agnate ties among the nobility in the early Middle Ages. According to these two historians, the five *genealogia*, aristocratic kindreds, who were assigned a double wergild in the eighth-century codification of Bavarian stem law, were agnate-affiliated groups.[41] Recently, however, Alexander C. Murray has shown that the *genealogia* were simply composed of individuals who had inherited, either through their father or mother, a share in an undivided property.[42] And Störmer's study of the name-giving customs of the Bavarian nobility has demonstrated that enates continued to be important in the twelfth and thirteenth centuries, particularly if the mother was an heiress.[43] For example, Count Siboto IV of Falkenstein was named after his maternal grandfather, Count Siboto of Weyarn, from whom the counts of Falkenstein obtained their comital dignity, and Siboto IV named his older son Kuno after his father-in-law, Count Kuno of Mödling.[44] As David Herlihy has pointed out, a patrilineal family structure was imposed in the High Middle Ages on a structure that did not distinguish between agnate and enate descent, and both patterns of descent can thus be detected in the same lineage.[45]

Geographical location and the relative strength of royal or princely authority affected the speed of the change from bilateral kinship to patrilineal lineage. Thus, changes in family structure occurred at different times in various parts of Europe. Duby discovered that the property of the six sips in the tenth-century Mâconnais, where royal and comital authority had broken down at an early date, had, by 1000, already been divided among twenty-four families.[46] While in the mid-eleventh century the Swabian nobles were establishing dynastic monasteries, a key indicator of the change in family consciousness, most of the Bavarian dynastic monasteries were established only in the first half of the twelfth century.[47] The best explanation

[39] Freed, *Counts of Falkenstein*, 45–49, 63–67. Also see Freed, "Diemut von Högl: Eine Salzburger Erbtochter und die erzbischöfliche Ministerialität im Hochmittelalter," *Mitteilungen der Gesellschaft für Salzburger Landeskunde*, 120/121 (1980/81): 646–56.

[40] Dopsch, *Geschichte Salzburgs*, pt. 1, pp. 361–67.

[41] Prinz, "Bayerns Adel im Hochmittelalter," *Zeitschrift für bayerische Landesgeschichte*, 30 (1967): 57–58; Störmer, "Adel und Ministerialität," 96–112, and *Früher Adel: Studien zur politischen Führungsschichte im Fränkisch-Deutschen Reich vom 8. bis 11. Jahrhundert*, Monographien zur Geschichte des Mittelalters, vol. 6 (Stuttgart, 1973), 44–51, 91, 212–13.

[42] Murray, *Germanic Kinship Structure: Studies in Law and Society in Antiquity and the Early Middle Ages*, Studies and Texts, vol. 65 (Toronto, 1983), 99–108.

[43] Störmer, "Adel und Ministerialität," 112–16.

[44] Freed, *Counts of Falkenstein*, 51.

[45] Herlihy, "The Making of the Medieval Family: Symmetry, Structure, and Sentiment," *Journal of Family History*, 8 (1983): 122.

[46] Duby, "Lineage, Nobility, and Knighthood," 67.

[47] Bosl, "Adel, Bistum, Kloster Bayerns im Investiturstreit," *Festschrift für Hermann Heimpel zum 70. Geburtstag am 19. September 1971*, Veröffentlichungen des Max-Planck-Instituts für Geschichte, vol. 36/2 (Göttingen, 1972): 1121–46. On the reform movement in Bavaria and Swabia, see Hans Hirsch, "The

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for this difference is that ducal authority was weak in eleventh-century Swabia, which became a major center of opposition to Henry IV, whereas Bavaria was in effect the Salian royal domain.[48] The change in family structure occurred even later in the remote Pinzgau in the Austrian Alps, where the two leading noble lineages, the Felbens and Walchens, adopted those surnames only around 1150.[49]

Neither Schmid nor his critics have paid much attention to the origins of the nobles' lordships, perhaps because there has been a widespread belief among German scholars since the 1940s that nobles were entitled to rule inferiors. While nineteenth-century liberal historians traced the roots of the medieval principalities to the delegation or usurpation of royal authority, primarily comital rights,[50] Otto Freiherr von Dungern argued in 1927 that the nobles had an innate right to rule. Their personal immunity from comital authority was extended to the people who lived on their domains and antedated the establishment of the Carolingian empire.[51] In 1933 Theodor Mayer maintained that France was the heir of the centralized Roman state and that even the feudal anarchy of the tenth century was based in theory on the delegation of royal authority, whereas Germany had been dualistic from its inception because the nobility had exercised extensive rights of government that were not derived from the crown. The basis of the nobles' authority was their innate right to reclaim and to rule the vast wilderness areas in Germany.[52] The theory of the nobility's autogenous regalian rights received its classic formulation in Schlesinger's 1941 study of the Carolingian comital structure in Thuringia, which was largely confined to the royal domains and which did not include the nobles' own lands and people.[53] The theory has been incorporated into postwar general histories of Germany. For instance, Josef Fleckenstein, a prominent member of the Freiburg School, wrote in 1976 that "the identification of feudalism with the rule of a noble class is . . . unsound, though it is often found in scholarly writing. In a strictly legal sense, rule by a noble class depends closely

Constitutional History of the Reformed Monasteries during the Investiture Contest," in Barraclough, trans., *Mediaeval Germany, 911–1250*, (Oxford, 1938), vol. 2, pp. 131–73; Hermann Jakobs, *Der Adel in der Klosterreform von St. Blasien*, Kölner historische Abhandlungen (hereafter, KhAbh), vol. 16 (Cologne, 1968), *Die Hirsauer: Ihre Ausbreitung und Rechtsstellung im Zeitalter des Investiturstreites*, KhAbh, vol. 4 (Cologne, 1961); Jakob Mois, *Das Stift Rottenbach in der Kirchenreform des XI.–XII. Jahrhunderts: Ein Beitrag zur Ordens-Geschichte der Augustiner-Chorherren*, Beiträge zur altbayerischen Kirchengeschichte, 3d ser., vol. 19 (Munich, 1953); Schmid, "Adel und Reform," 295–319; and Stefan Weinfurter, *Salzburger Bistumsreform und Bischofspolitik im 12. Jahrhundert: Der Erzbischof Konrad I. von Salzburg (1106–1147) und die Regularkanoniker*, KhAbh, vol. 24 (Cologne, 1975).

[48] Helmut Maurer, *Der Herzog von Schwaben: Grundlagen, Wirkungen und Wesen seiner Herrschaft in ottonischer, salischer und staufischer Zeit* (Sigmaringen, 1978), 129–217; and Max Spindler, ed., *Handbuch der bayerischen Geschichte*, volume 1: *Das alte Bayern: Das Stammesherzogtum bis zum Ausgang des 12. Jahrhunderts* (Munich, 1967), 227–46.

[49] *Monumenta Boica* (Munich, 1764), vol. 3, pp. 32–33, no. 94; and *Salzburger Urkundenbuch*, ed. Willibald Hauthaler and Franz Martin (Salzburg, 1916), vol. 2, pp. 497–99, no. 355b.

[50] See the historiographical survey in Schlesinger, *Entstehung der Landesherrschaft*, 1–5.

[51] Dungern, *Adelherrschaft im Mittelalter* (1927; reprint edn., Darmstadt, 1967), esp. 46–68.

[52] Mayer, "Geschichtliche Grundlagen der deutschen Verfassung: Festrede gehalten bei der Reichsgründungsfeier am 18. Januar 1933," *Schriften der Hessische Hochschulen, Universität Giessen*, vol. 1 (Giessen, 1933). This was translated by Barraclough; "The Historical Foundations of the German Constitution," *Mediaeval Germany*, 1–33. Barraclough incorporated Mayer's ideas in his own book; *The Origins of Modern Germany*, 6–11, 86–88.

[53] Schlesinger, *Entstehung der Landesherrschaft*, xi–xiii, 114–40.

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on the nobility's ownership of private property and not at all on the fief. Such private property was called an *allod* in the Middle Ages, and it would be more accurate to speak of 'allodialism,' something quite different from feudalism."[54]

While Mayer and Schlesinger emphasized the differences between France and Germany,[55] Werner, who has consistently stressed the common features in the French and German heritage, extended the concept of the autogenous lordship to Merovingian Gaul. Werner stated: "It is clear that the Roman senatorial nobility at least was there before the Merovingian king and hence before kingship. It is simply impossible to derive its rights from the former rights of the Germanic king. . . . This throws new light on the dualism of the new state. On the one hand, there was an attempt to restore as far as possible the Roman governmental structure. . . . On the other hand, we find episcopate and king, powerful Gallo-Romans and Merovingians, as partners from the start, both having a power not to be underestimated. Rule over the inhabitants of the private estates of the *potentes* was no more disturbed than it had been in Roman times. . . . Anyone who wants to find autochthonous noble rights will find them here earlier and better documented than anywhere else."[56] Werner has substituted a Roman for a Germanic origin of the nobility's autogenous rights.

The theory of the nobility's innate right to rule never gained universal acceptance. For example, in an article published in 1949, Otto Stolz maintained that territorial supremacy (*Landeshoheit*) did not develop in Upper Bavaria, Salzburg, and the Tyrol from a complex of private estates owned by nobles but was derived from ducal and comital authority supported by a territorial base.[57] In recent years the most important critics of the theory of autogenous lordships have been Mitterauer and Peter Feldbauer, who have traced the origins of the nobles' lordships to their possession of counties, the reclamation of royal forests, and, above all, the exercise of advocatorial rights over ecclesiastical immunities. For instance, Mitterauer showed that the basis of the Falkenstein lordship of Hernstein was five royal hides that Emperor Henry II granted to Tegernsee in 1020 and that Siboto IV's great-grandfather, Patto of Dilching, usurped as the abbey's advocate.[58] It is worth noting in this regard that Mayer's principal evidence for the

[54] Fleckenstein, *Early Medieval Germany*, 88. For similar views, see Alfred Haverkamp, *Aufbruch und Gestaltung Deutschland 1056–1273*, Die neue deutsche Geschichte (Munich, 1984), vol. 2, pp. 143–44.

[55] Schlesinger wrote: "Im Westen mag die Durchführung der staatlichen Ämterorganisation gelungen sein, denn hier untermischte sich der germanische Erobereradel mit der grundbesitzenden provinzial-römischen Aristokratie, die die Einordnung in das Ämterwesen gewöhnt war. Dem Osten blieb sogar der Begriff des Amtes fremd." *Entstehung der Landesherrschaft*, 120.

[56] Werner, "Important Noble Families," 144–45.

[57] Stolz, "Das Wesen der Grafschaft im Raume Oberbayern-Tirol-Salzburg," *Zeitschrift für bayerische Landesgeschichte*, 15 (1949): 68–109.

[58] Feldbauer, *Der Herrenstand in Oberösterreich: Ursprünge, Anfänge, Frühformen*, Sozial- und wirtschaftshistorische Studien (hereafter, SWhS) (Munich, 1972), esp. 188–205; Feldbauer, *Herrschaftsstruktur und Ständebildung: Beiträge zur Typologie der österreichischen Länder aus ihren mittelalterlichen Grundlagen*, volume 1: *Herren und Ritter*, SWhS (Munich, 1973), esp. 9–16; and Mitterauer, "Formen adeliger Herrschaftsbildung im hochmittelalterlichen Österreich: Zur Frage der 'autogenen Hoheitsrechte,'" *Mitteilungen des Instituts für österreichische Geschichtsforschung*, 80 (1972): esp. 292–93. Mitterauer wrote the introduction to *Herrschaftsstruktur und Ständebildung*. For a critique of Mitterauer's views about the origins of the ministerials' lordships, see Herwig Wolfram, "Zisterziens-

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significance of forest reclamation in the formation of lordships was a charter that Archbishop Frederick I of Salzburg (958–91), who started the colonization of the Pongau, forged and that Emperor Otto II confirmed. Salzburg's subsequent territorial supremacy in the Pongau was based on its jurisdiction over this forest.[59] That an archbishop, an "official" of the imperial church, reclaimed a forest without royal authorization hardly proves, however, that every noble had an innate right to do the same thing, and that Archbishop Frederick sought royal confirmation of his actions is surely significant. It is one thing to say that the nobles were perceived as having an inherited charisma that qualified them to rule over others; it is another to assert that the nobility could exercise authority legally without any higher authorization (that they did is another matter).

Finally, there is the bothersome fact that much of the discussion of the German nobility's innate right to establish lordships has focused on areas inhabited by Slavs. For example, Schlesinger discussed the Slavs who lived in central Germany, particularly in the Ottonian marks along the Elbe and Saale Rivers, but he minimized their influence on the subsequent history of the area. "One must start," he wrote, "with the mixture of German and Slavic elements in examining the constitution of the marks. Although the Germans were only a small minority, they were the lords of the land and were, in addition, the bearers of a far superior culture."[60] In his preface to the 1964 edition of *Die Entstehung der Landesherrschaft*, Schlesinger conceded that "the real difference between the eastern marks and the old German territories must be sought in the presence of a broad stratum of Slavic inhabitants. Their constitution needs to be investigated. . . . In 1941 I may perhaps have overestimated Avar, Viking, and even Frankish influences."[61] I cannot help but wonder whether German feelings about the Slavs in general and the political atmosphere in the 1930s and 1940s in particular contributed, consciously or not, to the popularity of the theory of the nobility's autogenous rights. In short, underlying the theory of the noble domination of medieval society is the usually unspoken assumption that nobles had an innate right to establish lordships without any formal authorization. But is that true?

KARL BOSL, WHO IS BEST KNOWN for his exhaustive study of the imperial ministerialage (*Reichsministerialität*), a topic he began investigating in 1943, has repeatedly tried to explain why the ministerial estate formed only in medieval Germany and why the rigid social structure that produced it survived so long east of the Rhine.[62] Twelfth-century Germans were already aware that their social

ergründung und Ministerialität am Beispiel Zwettls," *Jahrbuch des Vereins für Landeskunde von Niederösterreich*, 46/47 (1980/81): 17–39.

[59] *Salzburger Urkundenbuch*, 56–64, no. 34; 103–06, no. 57. See Mayer, "The Historical Foundations of the German Constitution," 22–24.

[60] Schlesinger, *Entstehung der Landesherrschaft*, 240, 210–42.

[61] *Ibid.*, xvi.

[62] Bosl, "Die Reichsministerialität als Element der mittelalterlichen deutschen Staatsverfassung im Zeitalter der Salier und Staufer," in Mayer, *Adel und Bauern*, 74–108, and *Die Reichsministerialität der Salier und Staufer: Ein Beitrag zur Geschichte des hochmittelalterlichen deutschen Volkes, Staates und Reiches*,

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structure was unique. In 1163 a monk of the Alsatian monastery of Eberheimmünster, who may very well have been a ministerial himself, wrote that, after Julius Caesar had defeated the Gauls, he rewarded the Germans who had assisted him by granting their princes the rank of senators and giving the lesser knights Roman citizenship. Before leaving for Rome, Caesar assigned the knights to the princes but stipulated that the princes were to be lords and defenders of the knights and were not to treat the knights as slaves and servants. Hence, the chronicler explained, the German knights, unlike those of other lands, were called servants of the royal fisc and princely ministerials.[63]

The tale is, first of all, an attempt to explain the restructuring of German noble society during the Hohenstaufen period. In 1180 Frederick Barbarossa recognized the rights of the *Reichsfürstenstand*, the tenants-in-chief of the crown.[64] The other counts, untitled noblemen (*Freiherren*), and powerful ministerials (such as the Bolanden and Liechtensteins), who had their own vassals and who exercised regalian rights in a territory they held in fief from the Reich or from an imperial prince, formed the estate of the lords (*Herrenstand*). But the differences between lords of noble and ministerial ancestry were not forgotten. The remaining noble lineages (if they had not died out or become ministerials as was the case in the principality of Salzburg), the great number of ministerials, and the vassals of the lords formed the estate of the knights. How rapidly these changes occurred depended in part on proximity to the French border—for instance, the ministerialage disappeared more rapidly in Brabant than in Guelders—and on the strength of the prince. The result was enormous variations in social structure, even between neighboring principalities, depending on local conditions. For example, both the archbishops of Salzburg and the dukes of Styria were strong princes, but

Monumenta Germaniae Historica, Schriften, vol. 10 (Stuttgart, 1950–51). Bosl has been a very prolific author. His most important articles, at least from the perspective of this article, are "Freiheit und Unfreiheit: Zur Entwicklung der Unterschichten in Deutschland und Frankreich," *Vierteljahresschrift für Sozial- und Wirtschaftsgeschichte* (hereafter, *VSWg*), 44 (1957): 193–219, "Das ius ministerialium: Dienstrecht und Lehnrecht im deutschen Mittelalter," *Vorträge und Forschungen*, 5 (1960): 51–94, "Über soziale Mobilität in der mittelalterlichen 'Gesellschaft': Dienst, Freiheit, Freizügigkeit als Motive sozialen Aufstiegs," *VSWg*, 47 (1960): 306–32, and "Vorstufen der deutschen Königsdienstmannschaft: Begriffsgeschichtlich-prosopographische Studien zur frühmittelalterlichen Sozial- und Verfassungsgeschichte," *VSWg*, 39 (1952): 193–214, 289–315. These articles and others, including Bosl's 1943 piece "Die Reichsministerialität," have been reprinted in Bosl, *Frühformen der Gesellschaft im mittelalterlichen Europa: Ausgewählte Beiträge zu einer Strukturanalyse der mittelalterlichen Welt* (Munich, 1964). All citations will be to this edition. In addition, see *Europa im Aufbruch: Herrschaft-Gesellschaft-Kultur vom 10. bis zum 14. Jahrhundert* (Munich, 1980), *Die Gesellschaft in der Geschichte des Mittelalters* (Göttingen, 1966), *Die Grundlagen der modernen Gesellschaft im Mittelalter: Eine deutsche Gesellschaftsgeschichte des Mittelalters*, 2 pts., Monographien zur Geschichte des Mittelalters, vol. 4 (Stuttgart, 1972), and *Mensch und Gesellschaft in der Geschichte Europas* (Munich, 1972). Sylvia L. Thrupp translated "Über soziale Mobilität"; "On Social Mobility in Medieval Society: Service, Freedom, and Freedom of Mobility as Means of Social Ascent," in Thrupp, *Early Medieval Society* (New York, 1967), 87–102. Reuter has translated chapter 5, part 3 of *Grundlagen der modernen Gesellschaft*; "'Noble unfreedom': The Rise of the *Ministeriales* in Germany," *The Medieval Nobility*, 291–311.

[63] *Chronicon Ebersheimense*, ed. Ludwig Weiland, Monumenta Germaniae Historica, Scriptores, vol. 23 (Hanover, 1874), 432.

[64] For information about the formation of the *Reichsfürstenstand*, see Julius Ficker, *Vom Reichsfürstenstande*, ed. Paul Puntschart, 2 vols. (1861–1921; reprint edn., 1984). Fritz Walter Schönherr provided an extremely useful summary and critique of Ficker's complex and incomplete book; *Die Lehre vom Reichsfürstenstande* (Leipzig, 1914).

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a powerful *Herrenstand* developed in Styria, which was composed in 1300 of three families of noble status and twenty-two of ministerial rank, because the Habsburg dukes were too preoccupied with imperial politics to profit from the extinction of lordly dynasties. In contrast, no *Herrenstand* formed in Salzburg because the archbishops focused their attention on the principality and acquired the lordships of families that died out.[65]

Second, the monk's tale reveals, as Bosl has pointed out, the ministerials' proud awareness of their importance and their embarrassment about their servile origins.[66] That embarrassment persisted until the twentieth century. In the 1920s, German scholars reluctantly accepted that the ministerials had been of servile origin and that service had been the major factor in their rise, but only after numerous attempts to prove the contrary had failed.[67] Bosl believed that the formation of the ministerial estate was part of a recurring pattern in German history. Pre-Carolingian Germanic society had been characterized by its rigid division between a charismatic nobility and a subservient peasantry. The nobles had viewed service as incompatible with their status as free men. The Germanic chieftains, including the Merovingians, had thus been forced to turn to the lower strata for retainers, for instance, the Merovingian *pueri regis*, antrustions, vassals, and the Carolingian imperial aristocracy. Because service ennobled, a service nobility—the *noblesse de robe* is a later example—appeared beside the Gallo-Roman senatorial aristocracy and the Germanic stem nobility, and the three groups gradually coalesced.[68]

This archaic (one of Bosl's favorite words)[69] social structure survived longer in Germany than in France because of France's Gallo-Roman and more deeply rooted Christian heritage, which stressed the virtues of service and fidelity to a superior. Indeed, in his comparative study of Italian, French, German, and Slavic society and culture between the tenth and fourteenth centuries, Bosl presented Germany as an example of the seignorial model.[70] German nobles continued to view any form of subordination, no matter how honorable or profitable, as demeaning. For instance, the twelfth-century Welfs were extremely proud of their ancestor Eticho, who had refused, allegedly, to speak to his son Henry with the Golden Plough after the latter had paid homage to the emperor, his brother-in-

[65] Freed, "The Origins of the European Nobility: The Problem of the Ministerials," *Viator*, 7 (1976): 228–33. In addition to the works cited there, see Dopsch, "Ministerialität und Herrenstand in der Steiermark und in Salzburg," *Zeitschrift des historischen Vereines für Steiermark*, 62 (1971): 21–31, and "Probleme ständischer Wandlung beim Adel Österreichs, der Steiermark und Salzburg vornehmlich im 13. Jahrhundert," in Fleckenstein, ed., *Herrschaft und Stand: Untersuchungen zur Sozialgeschichte im 13. Jahrhundert*, Veröffentlichungen des Max-Planck-Instituts für Geschichte, vol. 51 (Göttingen, 1977), 219–44; and Feldbauer, *Herrenstand in Oberösterreich*, and *Herrschaftsstruktur und Ständebildung*.

[66] Bosl, *Europa im Aufbruch*, 214. Also see Wolfram, "Zisterziensergründung und Ministerialität," 19–20.

[67] Freed, "Origins of the European Nobility," 215–222.

[68] Bosl, "Vorstufen der deutschen Königsdienstmannschaft," 238–55.

[69] See, for example, Bosl, *Europa im Aufbruch*, 15–16. Also see the titles of the various sections and subsections in *Grundlagen der modernen Gesellschaft*: "Die archaische Ganzheit und der archaische Mensch" (II, 3); "Christentum und Kirche im Aufbau der archaisch-feudalen Gesellschaft und Kultur" (IV, 3); and "Die Verwandlung der archaischen Gesellschaft durch soziale Mobilität und gesellschaftlichen Aufstieg" (V).

[70] Bosl, *Europa im Aufbruch*, 192.

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law, in order to obtain the lordship of Ravensburg. After hearing the tale, Duke Henry the Black of Bavaria is said to have sought Eticho's grave and to have built a church on the site.[71] Consequently, in the tenth and eleventh centuries, the German magnates, including the king, had to seek their bailiffs, household officials, administrators, and warriors, as the Merovingians and Carolingians had done earlier, among the servile population. Specifically, they turned to the very lowest stratum of the peasantry, the *servi proprii*, the serfs who did not possess their own tenures but who worked daily in a lord's household or on his demesne. These unattached serfs had daily personal contact with the lord they served and were at his complete disposal. Most important, unlike the *servi casati*, the serfs who had their own tenures, the *servi proprii* had freedom of movement, crucial according to Bosl, to upward social mobility in medieval society. Significantly, Bosl asserted that the class of the *servi proprii*, from which the German ministerials were recruited, was far larger in Germany than in France, where the servile population was more homogeneous. By the middle of the eleventh century, the rights of this new servile elite had already been codified, for example, in the famous ministerial code of Bamberg. Functionally, by the twelfth century the ministerials were nobles, but legally they remained serfs—what Bosl called "noble unfreedom" (*adelige Unfreiheit*), the peculiar situation that is reflected in the myth recounted by the monk from Eberheimmünster.[72]

Bosl has elaborated on the work of Hans Kuhn and František Graus to explain why the French, unlike the Germans, did not perceive an incompatibility between freedom and service. Kuhn maintained that the *comitatus* described by Tacitus, composed of freemen willing to fight until their death for their lord, was not a universal institution among the Germanic tribes. The German chieftains distrusted the well-born as potential rivals and preferred a servile retinue, a practice the Germans borrowed from the Celts, the original inhabitants of northern Europe.[73] Graus argued that fidelity was not a Germanic virtue but a biblical ideal promoted by the church to strengthen royal authority in the early Middle Ages.[74] Bosl concluded that the French nobles, influenced by deeply rooted Celtic and Christian concepts of fidelity and service to a superior, were more inclined to serve and to obey than their more recently Christianized German counterparts, the heirs of a purer Germanic tradition.[75] The process that played itself out in the Frankish realm in the early Middle Ages repeated itself, therefore, in Germany between the tenth and thirteenth centuries.

[71] *Annalista Saxo a. 741–1139*, ed. Georg Waitz, Monumenta Germaniae Historica, Scriptores, vol. 6 (Hanover, 1844), 764.

[72] Bosl employed the term "noble unfreedom," for example, in *Europa im Aufbruch* (p. 205) and in *Grundlagen der modernen Gesellschaft* (p. 190). See his "Freiheit und Unfreiheit," 180–203, "Das ius ministerialium," 301–08, and "Über soziale Mobilität," 169–79. For the text of the *Dienstrecht* of Bamberg, see Wilhelm Altmann and Ernst Bernheim, eds., *Ausgewählte Urkunden zur Erläuterung der Verfassungsgeschichte Deutschlands im Mittelalter* (4th edn., Berlin, 1909), no. 77.

[73] Kuhn, "Die Grenzen der germanischen Gefolgschaft," *Zeitschrift der Savigny-Stiftung für Rechtsgeschichte, Germanistische Abteilung*, 73 (1956): 1–83.

[74] Graus, "Über die sogennante germanische Treue," *Historica*, 1 (1959): 71–121.

[75] Bosl, "Das ius ministerialium," 291–94, and "Über soziale Mobilität," 162–67.

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As I pointed out in 1976, Bosl's psychological explanation, suggestive as it may be, does not explain why a hereditary ministerialage developed between the Loire and the Rhine only in those territories that belonged to the medieval empire—for example, in Brabant and Lorraine but not in Flanders and Champagne. It is hard to see why fidelity to one's lord should have been so highly esteemed on one side of a political boundary, drawn without regard for linguisitic differences, and not on the other. This suggests that there must have been political reasons for the use of the ministerials in the empire.[76]

The circumstances surrounding the formation of the ministerialage of the archbishops of Salzburg are fairly clear. The tenth-century archbishops needed men to repel the Magyars, to supply the military demands of the crown (in 980/81 Otto II ordered Archbishop Frederick I to send seventy heavily armed cavalrymen to Italy), and to participate in the monarchy's efforts to curb the autonomy of the dukes of Bavaria. At the same time the gradual dissolution of the sips, which Schmid described, made it increasingly difficult for the archbishop to find sufficient warriors among his own distant kinsmen. The problem was compounded in the case of Salzburg because the emperors deliberately selected non-Bavarians as archbishops in the eleventh century, when the duchy was under direct royal control. By 1050 the archbishop's servile retainers formed a quasi-hereditary estate. Compared to tenth-century France, Bavaria was remarkably stable in the Saxon and early Salian periods. Even the Magyar incursions lasted only half a century, and there were long truces between the raids. There was little room under such circumstances for a radical realtering of society. The major period in the rise of the servile elite was the era of the Investiture Contest, when the archbishops spent long periods in exile. During these years the ministerials adopted surnames and formed into lineages, and *ministerialis* became the standard designation for members of the estate in the 1120s. Still, the rapid advance of the proto-ministerialage during the Investiture Contest should not obscure the fact that the archbishops had been in firm control until 1077 and that Archbishop Conrad I (1106–47) restored order quickly in the archdiocese after he returned from exile in 1121.[77]

Although Bosl is undoubtedly correct in stressing the stability of German society, I object to labeling it as "archaic" in contrast to a "modern" French society,[78] though I confess to using the word "archaic" myself.[79] Such terminology strikes me as another manifestation of Germany's national inferiority complex. Nineteenth-century scholars wondered why medieval Germany, unlike France,

[76] Freed, "Origins of the European Nobility," 240.

[77] Dopsch, *Geschichte Salzburgs*, pt. 1, pp. 254–73; Freed, "Diemut von Högl," 585–90, 636–38, and "The Formation of the Salzburg Ministerialage in the Tenth and Eleventh Centuries: An Example of Upward Social Mobility in the Early Middle Ages," *Viator*, 9 (1978): 67–102; Spindler, *Handbuch der bayerischen Geschichte*, 204–21, 227–46; and Kurt Zeillinger, *Erzbischof Konrad I. von Salzburg 1106–1147*, Wiener Dissertationen aus dem Gebiete der Geschichte, vol. 10 (Vienna, 1968). There was a comparable development in the diocese of Freising; Flohrschütz, "Die Freisinger Dienstmannen im 10. und 11. Jahrhundert," *Beiträge zur altbayerischen Kirchengeschichte*, 25 (1967): 9–79.

[78] Bosl, "Das ius ministerialium," 320.

[79] Freed, "Origins of the European Nobility," 240.

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failed to achieve national unity; Bosl is asking the same question in another form. Why did Germany fail to develop a "modern," French-style, feudal structure, which in France provided the framework for the development of the nation-state? Historians have an obligation to explain the similarities and differences in political and social development in France and Germany. But why are we to assume that the French model was necessarily "better"? Such an interpretation presupposes that the unification of Germany was the ideal political solution for the problems of Central Europe.

A more profitable approach is to compare the social structures of different French and German territories without labeling one as "better" than the other. Let me cite an example. In his revised study of the Mâconnais, published in 1972, Duby demonstrated that the 105 laymen who owned property around Cluny and whose descendants were called knights in the twelfth century were the descendants of the men who had belonged to the noble sips of the tenth century.[80] *Miles* and *militiae* were used occasionally in the eleventh century as terms for the free vassals of the archbishop of Salzburg, but *miles* became in the twelfth century the standard designation throughout southeastern Germany for a servile warrior of a ministerial or of a noble of noncomital rank. No noble or prominent ministerial was identified as a knight in a source written within the archdiocese itself in the twelfth century. Not until the beginning of the fourteenth century in the principality of Salzburg did the few surviving lineages of prominent archiepiscopal ministerials finally adopt the designations *miles* or *Ritter* and merge with their own knights to form the *Ritterstand* of the later Middle Ages.[81] In *The Three Orders*, Duby placed the constitution of the French knighthood, which established the knights as an order, in the 1170s and concluded that most knights were "the heirs, quite simply, of the *milites* of the Frankish army."[82] But for *miles* to lose its servile connotations and for knights to be accepted as nobles took at least one hundred fifty years longer in Salzburg than in France.

How can we account for such terminological differences and such fundamental differences in social structure? Duby has repeatedly argued that the spread of chivalric culture, particularly the peace movement with its concept of the *miles Christi*, ennobled the concept of knighthood in eleventh-century France, and he cited knighthood as an example of a cultural pattern that the nobles adopted from their social inferiors.[83] In contrast, Fleckenstein asserted that Frederick Barbarossa introduced French chivalric ideas, including the concept of the

[80] Duby, "Lineage, Nobility, and Knighthood," 59–67.

[81] Joachim Bumke, *The Concept of Knighthood in the Middle Ages*, trans. W. T. H. and Erika Jackson (New York, 1982), 61–62; Dopsch, *Geschichte Salzburgs*, pt. 1, pp. 399–403; Paul Kluckhohn, *Die Ministerialität in Südostdeutschland vom zehnten bis zum Ende des dreizehnten Jahrhunderts*, Quellen und Studien zur Verfassungsgeschichte des Deutschen Reiches (Weimar, 1910), vol. 4, pt. 1, pp. 129–32; and Otto von Zallinger, *Ministeriales und milites: Untersuchungen ueber die ritterlichen Unfreien zunaechst in baierischen Rechtsquellen des XII. und XIII. Jahrhunderts* (Innsbruck, 1878). Also see my forthcoming article, "The Concept of Knighthood in the Archdiocese of Salzburg."

[82] Duby, *The Three Orders*, 155, 293–307.

[83] Duby, "The Diffusion of Cultural Patterns in Feudal Society," *The Chivalrous Society*, 171–77, "The History and Sociology of the Medieval West," *ibid.*, 85–87, "The Nobility in Medieval France," *ibid.*, 103–09, "Laity and the Peace of God," *ibid.*, 123–33, and "The Origins of Knighthood," *ibid.*, 158–70.

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Christian knight, into Germany after his marriage to Beatrice of Burgundy in 1156, whereas Joachim Bumke perceived the princes as the chief promoters of courtly culture.[84] The time lag is obvious in either interpretation, but, more important, chivalry was an idea that spread not to the higher but to the lower social orders in Germany. For that reason, barriers to the social mobility of the servile elite may have persisted, regardless of chivalric culture. Chivalry, secular or religious, was an esteemed foreign import in Germany, not an indigenous product. The spread of the Enlightenment to Germany provides an analogous example.

A final problem with Bosl's work is his insistence that the source of all freedom in the Middle Ages was service and submission to a lord who granted his dependents privileges. Bosl sought—for example, in his *Europa im Aufbruch*—the origins not only of the ministerials but also of the burghers and the free peasants within the *familia*.[85] The subtitle of one of Bosl's most important articles is "Service, Freedom, and Freedom of Mobility as Means of Social Ascent." Hans K. Schulze has suggested that such an interpretation of medieval social development is the direct consequence of accepting Heinrich Dannenbauer's thesis that Germanic society was composed of a mass of dependent peasants ruled by powerful nobles.[86] In such a world, freedom could only be a privilege granted by a gracious lord to his deserving subjects.

Bosl is not the only German scholar to harbor such views. Similar ideas about the relationship between service and freedom within medieval Christianity can be found in Tellenbach's classic book *Church, State, and Christian Society at the Time of the Investiture Contest*, first published in 1936. Tellenbach wrote: "The basis of this Christian conception [of freedom] was that every servitude was connected with a corresponding freedom; in short, to every 'free from' there corresponds a 'bound to.' The true content of any freedom is the very dependence of which it is the consequence, and from this derive twin conceptions of freedom—as something false, wretched and fugitive, or as a true, noble, and lofty ideal."[87] Herbert Grundmann reminded his fellow historians in 1957 that the linkage of freedom with service became popular during the Third Reich and that medieval men had perceived freedom as the opposite of lordship.[88] For instance, Eike von Repgow, the author of the *Sachsenspiegel*, the most important medieval German lawbook, argued around 1220 that there was no biblical justification for servitude; it was

[84] Bumke, *The Concept of Knighthood*, 143–44; and Fleckenstein, "Friedrich Barbarossa und das Rittertum: Zur Bedeutung der grossen Mainzer Hoftage von 1184 und 1188," in *Festschrift für Hermann Heimpel*, 1023–41. Fleckenstein's article has been reprinted; Arno Borst, ed., *Das Rittertum im Mittelalter*, Wege der Forschung, vol. 349 (Darmstadt, 1976), 392–418.

[85] Bosl, *Europa im Aufbruch*, 192–248. Also see his case study of Regensburg; *Die Sozialstruktur der mittelalterlichen Residenz-und Fernhandelsstadt Regensburg: Die Entwicklung ihres Bürgertums vom 9.–14. Jahrhundert*, Abhandlungen der Bayerischen Akademie der Wissenschaften, Philosophisch-historische Klasse, new ser., vol. 63 (Munich, 1966).

[86] Schulze, "Rodungsfreiheit und Königsfreiheit," 532. Also see Franz Staab, "A Reconsideration of the Ancestry of Modern Political Liberty: The Problem of the So-Called King's Freemen," *Viator*, 11 (1980): 51–69. Dannenbauer's article is cited in note 3.

[87] Tellenbach, *Church, State, and Christian Society at the Time of the Investiture Contest*, trans. R. F. Bennett (Oxford, 1940), 4.

[88] Grundmann, "Freiheit als religiöses, politisches und persönliches Postulat im Mittelalter," *Historische Zeitschrift*, 193 (1957): 23–53.

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based purely on force, imprisonment, and other illicit exercises of power.[89] As Schulze has pointed out, Grundmann's admonition has gone largely unheeded.[90]

AS GRUNDMANN'S COMMENTS INDICATE, many of the widely accepted theories about the medieval nobility that appear in postwar German historiography originated or became popular during the Third Reich: the noble domination of Germanic society, the existence of a Carolingian imperial aristocracy dependent on the crown, the nobility's autogenous regalian rights, and the linkage of freedom with service. Let me stress that this does not necessarily invalidate these propositions, though a reexamination may be in order, or indicate that the historians who espoused such views were Nazis or even sympathetic to that regime. After all, Dannenbauer encountered considerable difficulties in 1941 in publishing his article about the aristocratic domination of Germanic society; Lintzel reminded his colleagues at considerable personal risk in 1934 that a historian had to retain his objectivity even in a period of national revival; and Tellenbach ended his *Church, State, and Christian Society* with the admonition that all Christians, Catholic and Protestant, draw a sharp distinction between the Kingdom of God and all earthly kingdoms.[91]

All historians, not only Germans, are influenced by political developments in their own time, but German historiography has always had a tendency to be *zeitbedingt*—to use that marvelous German euphemism—and not only in the Nazi period. As Werner pointed out, German historians may not have been Nazis themselves, but their conservative nationalistic views had much in common with Nazi ideology.[92] Werner's own postwar articles about the Carolingian nobility often sound like an attempt to place the origins of the Common Market in the ninth century. And, specifically to the point of my analysis, Reuter argued that, while the history of medieval England has been "politically innocuous . . . for at least two centuries," this has not been true of German medieval history.[93] For example, Baron Karl vom und zum Stein, the Prussian statesman and reformer, was the creator of the Monumenta Germaniae Historica. Its well-known motto, "Sanctus amor patriae dat animum," provides an insight into vom Stein's motives for promoting the publication of medieval German sources.[94] The famous debate between Heinrich von Sybel and Julius Ficker in the nineteenth century about the medieval empire was in essence an argument about the respective merits of a *kleindeutsch* versus a *grossdeutsch* unification of Germany.[95]

[89] Haverkamp, *Aufbruch und Gestaltung*, 286.
[90] Schulze, "Rodungsfreiheit und Königsfreiheit," 529.
[91] Dannenbauer, "Adel, Burg und Herrschaft," preface to 1956 edition; Lintzel, "Zur Beurteilung Widukinds und Karls des Grossen," *Vergangenheit und Gegenwart*, 24 (1934), reprinted in Walther Lammers, ed., *Die Eingliederung der Sachsen in das Frankenreich*, Wege der Forschung, vol. 185 (Darmstadt, 1970), 81–82; and Tellenbach, *Church, State, and Christian Society*, 167–68. See the comments of Lammers; *Die Eingliederung*, x. Also see Werner, *NS-Geschichtsbild*, 48–49, 56–59.
[92] Werner, *NS-Geschichtsbild*, 97.
[93] Reuter, "A New History of Medieval Germany," 440.
[94] David Knowles, *Great Historical Enterprises* (London, 1962), 65–74.
[95] Heinrich Hostenkamp, *Die mittelalterliche Kaiserpolitik in der deutschen Historiographie seit v. Sybel und Ficker*, Historische Studien, vol. 255 (Berlin, 1934).

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James J. Sheehan has asserted that all German historians, though they may disagree about everything else, "still accept the historiographical legitimacy of the settlement of 1871." According to Sheehan, that historiographical tradition rests on two not necessarily reconcilable propositions: the existence of a German folk as a distinct entity and Prussia's destiny to unify it. As a recent example of an unthinking attempt to reconcile these ideas, Sheehan cited Bosl's statement that "Bismarck fulfilled the will of the Volk from above." "In response to this extraordinary statement," Sheehan commented, "We can only ask, 'What Volk?' Surely not the Reich's Polish, Danish and French minorities. And what about the supporters of the Guelph monarchy, the Saxon particularists, the south Germans and Austrians?" Ultimately, Bosl's repeated description of medieval German society as "archaic" rests on the unquestioned assumption that the unification of Germany in the form of the Second Reich was the inevitable destiny of the German folk and that the long delay in achieving that goal was the consequence of Germany's "archaic" social structure.

Furthermore, Sheehan concluded that this *kleindeutsch* domination of German historiography has affected the interpretation of modern German history in a number of ways—some of which are also applicable, I think, to the study of medieval Germany. First, German historians have focused on politics at the national level, which had led some of them to treat even nonpolitical topics from a political perspective and to ignore those subjects that do not readily lend themselves to such an approach. For example, studies of the economic aspects of the *Reichsgründung* have dealt with the "economic dimensions of nation-building" rather than with "economic development" per se, and, until recently, modern German historians have paid virtually no attention to the history of women and family life. Second, local and regional studies have had little influence on the interpretation of national history. As Sheehan has observed, "Characteristically, this [German regional history] is history written and read by those in the locality itself; it is of interest only to people who share the experiences of the *Heimat*. It is difficult, therefore, to think of works on regional history which have had a major impact on German national historiography comparable to Vann Woodward's book on the American south or Georges Lefebvre's on the Nord."[96]

In sum, the German interest in the medieval nobility has remained essentially political. Tellenbach studied the role of the imperial aristocracy in the formation, governance, and dissolution of the Carolingian empire; Schmid explored the relationship between the change in noble self-consciousness and the formation of the territorial lordships; and Bosl examined the involvement of the imperial ministerials in the Hohenstaufen revival. Bosl's classic study of the imperial ministerialage is, after all, subtitled *A Contribution to the History of the High Medieval German Folk, State, and Reich*. Schmid may have made us aware of the change in family structure that occurred around the year 1000 and its ramifications for the German constitution, but it was Duby who investigated its impact on the noble

[96] Sheehan, "What Is German History? Reflections on the Role of the *Nation* in German History and Historiography," *Journal of Modern History*, 53 (1981): 3, 16, 13, 11.

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family itself—namely, the preference shown to the eldest son at the expense of his sisters and younger brothers. Perhaps it is Duby's broader approach that has led American university presses to publish so many translations of his books and articles and so little of Schmid's work.

In addition, in spite of the postwar popularity of *Landesgeschichte* among German medievalists, I know of no German study of a specific region comparable, say, to Duby's *La société aux XI^e et XII^e siècles dans la région mâconnaise* (1953) or Guy Devailly's *Le Berry du X^e siècle au milieu du XIII^e: Etude politique, religieuse, sociale et économique* (1973). Ironically, the only study of a German territory similar to the studies produced by the *Annales* school is Philippe Dollinger's *L'évolution des classes rurales en Bavière depuis la fin de l'époque carolingienne jusqu'au milieu du XIII^e siècle* (1949), but it is concerned, as the title indicates, with the peasantry alone. In German historiography, medieval *Landesgeschichte* remains the story of how a prince created his state by subduing rival noble families, by employing ministerials as instruments of princely authority, and by promoting land clearance and the founding of cities. We need monographs that will investigate such topics as the composition of the nobility, changes in family structure, the rights of women and younger sons, the age of marriage, the formation and rise of the ministerialage, and the causes of family extinction. This is a formidable assignment, but it is not beyond the capacity of German *Gründlichkeit* and *Fleiss*. Once these types of historical studies have been done, we may be able to make comparisons between medieval French and German societal development without labeling one structure as preferable to the other.

Above all, the time has come for medievalists as well as for scholars of modern Germany, in studies of the local as well as the national level, to abandon the primacy of politics and to accept the diversity of the German-speaking world's rich heritage in all of its aspects. The legacy of Bismarck's defunct state does not have to shape forever our understanding of a thousand years of German history.

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AHR Forum

Mission to the Jews and Jewish-Christian Contacts in the Polemical Literature of the High Middle Ages

DAVID BERGER

SPREADING THE GOOD NEWS has been a principal objective of Christianity since its infancy. Nevertheless, after the initial Jewish rejection of the Christian message, the expansionism of the church was directed mainly toward the pagan world, and it is by no means clear that even those patristic works that were directed *adversus Judaeos* were marked by realistic missionary objectives.[1] Jews, moreover, were granted unique toleration in Christian Europe on the theological grounds that they served, however unwillingly, as living testimony to Christian truth and that their conversion at the end of days was required by biblical prophecy. At the same time, no one doubted that the acceptance of Christianity by individual Jews was devoutly to be wished. Thus, at its core, the fundamental theory governing Jewish status in early medieval Europe was marked by tension and ambivalence—a result of the contradiction between the theoretical goals of a universal Christian mission and an argument for toleration that came close to discouraging Jewish conversion.

Christian polemic against Jews is a crucial genre for the study of missionary intentions, and the theoretical tension that I noted is clearly reflected in the assessment of that literature in the standard study of Jewish-Christian relations before the First Crusade. Bernhard Blumenkranz devoted much of his *Juifs et Chrétiens dans le monde occidental, 430–1096*, to the issues of polemic and mission.[2] On the one hand, he indicated that pre-crusade polemic against Jews was intended for Christian disputants in a context that did not involve a direct and immediate mission.[3] On the other hand, he stressed the persistence of the missionary ideal as a motive for polemical activity: Christians were impelled by a natural desire to persuade others of the truth, by the aspirations of believers in a majority faith to make that faith the exclusive one, and by the great Christian expectation of seeing all humanity "assembled under the scepter of Christ." To a significant degree,

[1] See David Rokeah, *Jews, Pagans, and Christians in Conflict* (Jerusalem, 1982), 40–48. Also see my brief discussion in David Berger, *The Jewish-Christian Debate in the High Middle Ages: A Critical Edition of the Nizzahon Vetus with an Introduction, Translation, and Commentary* (Philadelphia, 1979), 4–5.

[2] Blumenkranz, *Juifs et Chrétiens dans le monde occidental, 430–1096* (Paris, 1960).

[3] *Ibid.*, 75. On Blumenkranz's complex position, see, especially, note 72, below.

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then, Blumenkranz perceived pre-crusade literary polemics as the result of a missionary objective.[4]

If this assessment of the relatively sparse polemical literature written before 1096 is sound, the much richer material from the following century should reflect a similar motive. But, even if one remains skeptical about a significant missionary impulse in the early period, a number of considerations require a fresh and careful look at the possibility of missionary objectives in the elusive twelfth century. First, the resurgence of polemic, which produced almost twenty works from the late eleventh through the twelfth century, suggests prima facie a more aggressive Christian attitude toward conversion of Jews. Second, we know that mission to the Jews had become an important goal of many Christians by the mid-thirteenth century,[5] and the suddenly numerous polemical works of the previous century are a tempting and reasonable place to look for the roots of this phenomenon. Third, the widely admired First Crusade, with its bloody attempt at the forcible conversion of the Jewish communities in the Rhineland, could have been responsible for placing mission to the Jews on the agenda of a newly expansionist and assertive Christendom, and the upsurge of anti-Jewish works could be understood as a product of that expansionism. Finally, the crucial turning point in the Christian attitude toward mission to Islam occurs in the twelfth century. Benjamin Z. Kedar's recent study of this question has pointed to an utter indifference to converting Muslims during the early Middle Ages that gave way by the mid-twelfth century to a strong feeling that they should be made Christian. From that point on, there appears to have been little question of the desirability of mission, and the controversy was confined to the debate between advocates of force and of persuasion.[6]

This question is but one aspect of the broader challenge that the twelfth century has posed for Jewish historians. The early Middle Ages, with the exception of Visigothic Spain, were a period of relative peace and opportunity for Europe's Jews. Only in the eleventh century did some distinct signs of deterioration in their status begin to emerge, although questions remain about both the extent and the cause of that decline.[7] The century culminated, of course, in the crusade, and by the late thirteenth century the Jews of Northern Europe were subject to expulsions

[4] *Ibid.*, 67–68, 152, 216.

[5] Although I have some reservations about Jeremy Cohen's conclusions concerning the shift from tolerance to intolerance of Jews in the thought of the thirteenth- and fourteenth-century friars, his thorough description of their missionary objectives is correct. See Cohen, *The Friars and the Jews* (Ithaca, N.Y., 1982). For my review, see *AHR*, 88 (1983): 93.

[6] Kedar, *Crusade and Mission: European Approaches toward the Muslims* (Princeton, N.J., 1984), esp. 57–74.

[7] See Lena Dasberg, *Untersuchungen über die Entwertung des Judenstatus im 11. Jahrhundert* (Paris, 1965). Compare the remarks of Amos Funkenstein; "Basic Types of Christian Anti-Jewish Polemics in the Later Middle Ages," *Viator*, 2 (1971): 377. Also see Gavin Langmuir, "From Ambrose of Milan to Emicho of Leiningen: The Transformation of Hostility against Jews in Northern Christendom," *Gli Ebrei nell'Alto Medioevo, Settimane di Studio*, 26 (Spoleto, 1980): 313–68; and Avraham Grossman, *Hakhmei Ashkenaz HaRishonim* (Jerusalem, 1981), 12–13, 163. Compare the remarks in my review; *Tarbiz*, 53 (1984): 480. Also see Robert Chazan, "1007–1012: Initial Crisis for Northern European Jewry," *Proceedings of the American Academy for Jewish Research*, 39 (1972): 101–18; and Kenneth R. Stow, *The "1007 Anonymous" and Papal Sovereignty: Jewish Perceptions of the Papacy and Papal Policy in the High Middle Ages* (Cincinnati, Ohio, 1984).

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and persecution. Can a more or less straight line be drawn from the First Crusade to the expulsions, or was it only in the thirteenth century that relatively new forces emerged that moved the history of medieval European Jewry toward its tragic denouement? In the twelfth century, the Second Crusade swept through the Rhineland, the ritual murder accusation was born, and yet the Jewish community continued to function in a hostile but relatively stable environment. From a cultural perspective, the period was one of dazzling achievement. Even the acute contemporary observer would not have seen a people poised at the edge of a precipice.[8]

With regard to the question of mission, the historiographical problem posed by the twelfth century emerges in all of its tantalizing ambiguity in an intentionally cautious and ambivalent formulation by Salo Baron. "In the Roman and Byzantine empires, and even in western Europe before the age of the Crusades, the numerous tracts 'Against the Jews' primarily had Christian audiences in mind. Now, on the contrary, the Church viewed the apologetic literature as but another weapon in its march toward world domination. The new offensive, seized particularly by the preaching orders, also infused new vigor and introduced novel facets into the polemics which, together with the vastly expanding missionary sermons and oral disputations, tried to persuade the Jews of the 'foolishness' of their stubborn perseverance."[9] At first, this passage suggests that a change in Christian attitude occurred at the beginning of "the age of the Crusades," but almost immediately the emphasis shifts to "the preaching orders," which belong to the thirteenth century. Once again, the twelfth century is left in a sort of limbo. Was it a watershed in the use of polemic as a weapon in the church's "march toward world domination," or does this questionable distinction belong to the age of the friars?[10]

I believe this question can be answered unequivocally. Despite the proliferation of Christian polemics in the late eleventh and twelfth centuries, the evidence is overwhelming that these works were not rooted in a new or continuing missionary impulse. An examination of the reasons that polemicists gave for writing their tracts reveals a remarkable need to apologize for engaging in an activity considered improper on ideological grounds, and, even when there is no apology, hesitation, or refusal, the reasons given almost invariably do not include the idea that Christians should attempt to proselytize Jews.

If this conclusion is correct, then two potential explanations for the upsurge of Christian polemic remain. First, the primary impulse for this literature may have come from outside the arena of Jewish-Christian relations and resulted, instead, from the overall cultural renaissance of the late eleventh and twelfth centuries. Since no Christian engaged in a careful examination of the sacred texts and doctrines of Christianity could have avoided a confrontation with Judaism,

[8] For a vigorous argument that 1096 was not a watershed in Jewish history, see Robert Chazan's *The First Crusade and European Jewry* (forthcoming).

[9] Baron, *A Social and Religious History of the Jews* (2d edn., New York, 1965), vol. 9, p. 101.

[10] Amos Funkenstein did not take a clear position on this question in his studies of twelfth-century Christian polemic. See "HaTemurot BeVikkuaḥ HaDat shebein Yehudim LeNoẓrim BaMeah HaYod-Bet," *Zion*, 33 (1968): 125–44, and "Basic Types," 373–82.

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apologetic writings could have been an inevitable consequence of an internal Christian dynamic.[11]

On the other hand, the impetus for these works may lie in Jewish-Christian interaction of the most vibrant sort. There is no longer any question that some Jews in Northern Europe were involved in discussions of biblical and other issues with Christian scholars.[12] The polemical literature contains considerable evidence that ordinary Jews and Christians held lively, informal debates about sensitive religious matters, and the authors of Christian polemics speak of the need for a response to aggressive Jewish questions. Such debates no doubt predated the High Middle Ages, but they may well have intensified as a result of the growing intellectual sophistication engendered by the cultural revolution that transformed both Jewish and Christian society in this period. The renaissance of the High Middle Ages surely facilitated the literary expression of these confrontations by both sides. The Christian assertions that Jews posed provocative questions with frequency and vehemence must be taken seriously. Christians were not confronting Jewish missionaries, but they faced a genuine, vigorous challenge from a proud and assertive Jewish community.[13]

ELEVENTH- AND TWELFTH-CENTURY CHRISTIAN intellectuals had profound reservations about mission to the Jews. Perhaps the most striking illustration of this position is the outright refusal of Adam of Perseigne (d. 1203) to accede to a friend's request that he write an anti-Jewish polemic. First, it seemed to him that his friend was motivated more by the desire to dispute than by zeal for the truth; second, Christians, he believed, should not be contentious on general principles (2 Tim. 2: 23–24). Third—and for us most important—the Jews would remain blind and hard-hearted "until the fulness of the nations will come in" (Rom. 11:25). The famous verse from Romans, then, predicts the futility of missionary efforts and may even intend to discourage them; Jewish conversion is reserved for the *eschaton*. Later, Adam added a further consideration: Christians should not pollute themselves with discussions of falsehood but should study Christian doctrines with pure heart and hands and simple eyes.[14] Since Adam could have written a brief compendium of standard anti-Jewish arguments with little more effort than it took him to write this letter, I doubt that he was concocting excuses for a refusal motivated by laziness; this is a genuine, antimissionary ideology.

In one of the earliest polemics of the period, hesitation is followed by acceptance of responsibility. Peter Damian (d. 1072) mentioned Jewish conversion as a reason

[11] At one time, I regarded the influence of internal Christian developments as slightly more central to the upsurge in polemic than I do now. See my brief remark in *The Jewish-Christian Debate*, 16.

[12] For an important summary of the evidence, see Aryeh Grabois, "The *Hebraica veritas* and Jewish-Christian Intellectual Relations in the Twelfth Century," *Speculum*, 50 (1975): 613–34.

[13] I discuss the state of this question later in this article.

[14] Adam of Perseigne, *Epistola ad amicum*, *Patrologia Latina*, ed. J. P. Migne (hereafter, *PL*) 211: 653–59. It bears noting that Adam considered a bad Christian worse than a Jew, who acted in ignorance; *ibid.*, 657, 659. This reference should be added to Jeremy Cohen's valuable discussion of the question of Jewish ignorance and culpability in Christian thought. See Cohen, "The Jews as the Killers of Christ in the Latin Tradition, from Augustine to the Friars," *Traditio*, 39 (1983): 1–27.

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for writing polemic, but almost as an afterthought following an exhortation to concentrate on more important things than arguing with Jews. Damian was responding to a request from a churchman named Honestus to provide material refuting Jewish arguments, and he began by suggesting that, "if you wish to be a soldier of Christ and fight for him courageously, then take up arms . . . against the vices of the flesh, the contrivances of the devil—an enemy who will clearly never die—rather than against the Jews, who will soon be virtually destroyed from the face of the earth." Nevertheless, he agreed to provide the material because it was disgraceful (*inhonestum*) to remain silent while Christianity was insulted, such silence could arouse doubts in Christian minds, and, finally, Jews might be converted by well-presented Christian arguments.[15] A reluctant missionary indeed.

Peter of Blois (d. 1200) provided an even stronger prolegomenon before finally acquiescing and writing his polemic. He addressed the work to a Christian who complained that he was surrounded by Jews and heretics and was unequipped to answer the tricky arguments raised by the Jews in their disputations. It was unwise, Peter said, for someone without good polemical aptitude to debate with a heretic or a Jew; such disputes, in fact, tended to turn the inexperienced Christian into a heretic himself. And it was surely absurd to debate a subject like the Trinity. In effect, Peter argued that Christians need not worry about educating the heretic or Jew: beasts were not permitted to touch Mount Sinai, and pearls were not to be cast before swine.[16] Moreover, if one defeats an enemy of the cross in debate, he will in any event not convert in his heart. As for the Jews, they cannot be converted because God has set them an end that cannot be advanced. One might, it is true, make an occasional convert, but the rest will persist in their stubbornness.[17] Apparently, the missionary enterprise was not sufficiently justified by the handful of souls that might be saved.

Peter, of course, did relent and write polemic, and suspicious historians may be tempted to conclude that this introductory show of reluctance is a disguise for missionary zeal. In determining twelfth-century attitudes toward mission, however, what he wrote is decisive, and underlying motives are of secondary significance as signposts of future developments. In a sense, the point would even be strengthened, since Peter was apparently embarrassed to be pursuing an objective that any Christian would have been expected to applaud. It seems probable that discouraging mission to the Jews was an ideology that arose as a rationalization to explain centuries of relative indifference to Jewish conversion or

[15] Peter Damian, *Antilogus contra Judaeos*, *PL* 145: 41. See my discussion in "St. Peter Damian: His Attitude toward the Jews and the Old Testament," *Yavneh Review*, 4 (1965): 83–84. Blumenkranz's references to this work illustrate his tendency to emphasize missionary motivations. He first cited Damian's hope for conversion, two pages later he indicated the rather different need to assist Honestus, and considerably later he referred to the *Antilogus* without qualification as a "missionary work"; *Juifs et Chrétiens*, 69, 71, 153.

[16] In light of the context of this phrase in the Gospels, its use as an argument against preaching to Jews is painfully ironic.

[17] Peter of Blois, *Contra perfidiam Judaeorum*, *PL* 207: 825–27.

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even as a direct reaction to protracted Jewish stubbornness. Nevertheless, the reluctance to proselytize among Jews remains both surprising and significant.

Peter relented, he said, because the request came from a person beset (*obsessum*) by Jews and heretics. A similar defensive motive is proffered in an anonymous twelfth-century polemic whose author maintained that he wrote for simple people and in simple faith, not for the sake of dialectical disputations. Jews, he said, should not be able to mock Christian ignorance (*imperitia*)—those Jews "who taunt [*insultant*] us all day and say with Goliath, 'Choose someone from among you who will engage in a one-on-one battle with us.'"[18] Similarly, Walter of Châtillon introduced his polemic with the remark that the Jews "not only fail to acquiesce in the truth of the new grace but even, like retrograde planets, attempt to oppose the firmament of our faith and pose objections to Christians from the authority of the Pentateuch. Hence, mindful of their ignorance [again, *imperitia*, this time about the Jews], we decided to write a book with arguments so compelling that even . . . [an] ass will not be able to contradict them."[19] Once more a defensive motive appears. Whether the conditions described by Peter, Walter, and the anonymous author of the *Tractatus* reflect historical reality is an issue to which I shall presently return, but the reiteration of such defensive claims underscores the absence of explicit missionary goals.

Rupert of Deutz (d. 1135), like Adam and the two Peters, wrote in response to a request. Rudolph of St. Trond, at that time abbot of St. Pantaleon in Cologne, is reported to have had frequent contact with Jews, and he not only requested Rupert's polemic but subsequently wrote a letter asking for material that would demonstrate the triune God and the incarnation and discuss the evidence from Gen. 49:10.[20] Rudolph may have been interested in mission, but Rupert, who did not accede to the request immediately, would not have written polemic on his own initiative.[21]

In addition to Peter Damian's *Antilogus*, the other anti-Jewish polemics written in the eleventh century also contain motives that have little to do with mission. Fulbert of Chartres maintained that his intention was to speak of the errors of

[18] *Tractatus adversus Judaeum*, *PL* 213: 749. The effectiveness of Jewish debaters is also attested in Bartholomew of Exeter's unpublished "Dialogus contra Judaeos" (early 1180s), which warns against engaging in public controversies with them. At the same time, Bartholomew remarked (if only in a subordinate clause) that "we hold discussions with them for their own salvation." There is, then, a missionary intention blunted by fear of the consequences of disputation. For the relevant passage, see R. W. Hunt, "The Disputation of Peter of Cornwall against Symon the Jew," in R. W. Hunt *et al.*, eds., *Studies in Medieval History Presented to Frederick Maurice Powicke* (Oxford, 1948), 147–48.

[19] Walter of Châtillon, *Tractatus sive dialogus contra Judaeos*, *PL* 209: 424–25.

[20] "Ein Brief des Chronisten Rudolph von St. Trond an Rupert von Deutz," *Neues Archiv*, 17 (1892): 617–18. This letter influenced the writing of *De glorificatione Trinitatis* and perhaps part of *De gloria et honore Filii hominis*. See J. H. Van Engen, *Rupert of Deutz* (Berkeley and Los Angeles, 1983), 246–47, 354–55; and G. Niemeyer, ed., *Hermannus quondam Judaeus opusculum de conversione sua*, Monumenta Germaniae Historica: Quellen zur Geistgeschichte des Mittelalters, vol. 4 (Weimar, 1963), 5 n. 3, and 41–43. Rudolph's letter sounds like the request of a man who had read Fulbert of Chartres's *Tractatus contra Judaeos*, which deals precisely with the topics specified, and found it inadequate in real discussions with Jews.

[21] Rupert of Deutz, *Anulus sive dialogus inter Christianum et Judaeum*, in M. L. Arduini, *Ruperto de Deutz e la controversia tra Cristiani ed Ebrei nel secolo XII* (Rome, 1979), esp. 184. The work is also in *PL* 170: 559–610.

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nonbelievers in general; he began with Jews because they agreed with Christians in their monotheistic faith and disagreed with respect to several clearly defined issues: the Trinity, the divinity of the Messiah, and whether or not he had come.[22] Gilbert Crispin introduced his enormously influential disputation by saying that it reflected amicable discussions that he had had with a Jewish acquaintance who came to him frequently on business and other matters, at which times they conversed about the Scriptures and issues of faith. He did note that a Jew present at such discussions converted and became a monk, but he seemed to regard this as something of an unanticipated bonus rather than the purpose of the conversation and gave no indication that his book was to be used in any special effort to convert Jews.[23]

In the following century, an author once thought to be William of Champeaux produced a sharper version of Crispin's disputation and introduced a reference to missionary intentions into his paraphrase of Crispin's introductory passage. "I was acquainted with a certain Jew because of a business affair; as time passed, I was moved by love to urge him frequently to abandon Judaism and become a Christian."[24] Although this work does not, of course, reflect a real experience, the author's remark is not insignificant, but his assertion is limited to a specific Jew whom he was allegedly motivated to convert because of personal friendship. No interest in a broader mission is either stated or implied. At the end of a work directed mainly at Christian heretics, Alan of Lille appended a chapter on the Jews also derived largely from Crispin. In this case, the structure as well as the content make it abundantly clear that the author, who also added a chapter on Islam, did not write out of a missionary zeal directed at Jews.[25]

Similar attitudes appear in two early twelfth-century polemics concerning the incarnation. Odo of Cambrai addressed his work to a monk who had been present at a lecture by Odo on the incarnation and had urged him to put it in writing. Odo was finally persuaded to do so, but, before he wrote his book, he had a discussion on the subject with a Jew. Consequently, it seemed appropriate to Odo to record his remarks in the form of a dialogue. "Now, then, I invoke the Holy Spirit so that whatever inspiration it gave me for the purpose of convincing a Jew it might give me once again for the instruction of a faithful monk."[26] Odo then described how the Jew Leo visited him after his midday nap and initiated the discussion that he recorded. Once again, the question of historicity can be postponed; the immediate point is that Odo proffered no missionary intention at all and explicitly directed

[22] Fulbert of Chartres, *Tractatus contra Judaeos*, *PL* 141: 308.

[23] *Gisleberti Crispini disputatio Judaei et Christiani*, ed. Bernhard Blumenkranz (Utrecht, 1956), 27–28. The work is also in *PL* 159: 1005–36.

[24] Pseudo–William of Champeaux, *Dialogus inter Christianum et Judaeum de fide Catholica*, *PL* 163: 1045. On the tone of this work, see Israel Lévi, "Controverse entre un Juif et un Chrétien au XI[e] siècle," *Revue des Etudes Juives*, 5 (1882): 244.

[25] *De fide Catholica contra haereticos*, *PL* 210: 305–430, bk. 3, *PL* 210: 399–422. See my "Gilbert Crispin, Alan of Lille, and Jacob ben Reuben: A Study in the Transmission of Medieval Polemic," *Speculum*, 49 (1974): 34–47; and M. H. Vicaire, "'Contra Judaeos' méridionaux au début du XIII[e] siècle: Alain de Lille, Evrard de Béthune, Guillaume de Bourges," in M. H. Vicaire and B. Blumenkranz, eds., *Juifs et judaïsme de Languedoc* (Toulouse, 1977), 269–87.

[26] Odo of Cambrai, *Disputatio contra Judaeum*, *PL* 160: 1103.

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his work to a Christian audience motivated by a desire to understand the incarnation.

Guibert de Nogent's *Tractatus* on the incarnation *contra Judaeos* was actually directed against a count of Soissons who, Guibert said, cultivated the views of Jews and heretics. To Guibert, it was tolerable when someone who never accepted Christianity rejected it; the Jews, after all, grew up with this attitude, implanted in them since their forefathers crucified Jesus. What was intolerable was for people who called themselves Christians to attack the faith. To make matters worse, the count dared to proclaim nefarious ideas that Jews themselves were afraid to utter aloud. The Jews, in fact, considered him insane, because he extolled their sect while ostensibly following Christianity.[27] Although Guibert said that four years after writing the book he used it to strengthen the faith of a Jewish convert, there are no missionary overtones whatever in the reason he gave for its composition. The Jews, in fact, are used almost as a foil for the real object of Guibert's attack, and the implication is that he would not have written to expose the longstanding errors of a tolerated Jewish community.

Even the three polemics by pre-thirteenth-century converts to Christianity do not reflect the systematic missionary zeal that we might expect. The work by "Samuel of Morocco," though it deals with some broader issues, is couched as an explanation of the exile addressed to a Jew named Isaac, and it is hard to decide whether the work is a polemic with a limited missionary purpose or an *apologia pro conversione sua*.[28] Petrus Alfonsi explicitly described the dialogue with his own former Jewish persona as a reaction to attacks questioning the motives and sincerity of his conversion,[29] and the fascinating little book by Herman of Cologne is an autobiographical account of his experiences on the road to conversion rather than a true polemic.[30]

A final work that fits this pattern is essentially *sui generis*. Peter Abelard wrote a dialogue involving a philosopher, a Christian, and a Jew in which the relevant discussion takes place between the Jew and the philosopher, not between the Jew and the Christian. The irenic tone as well as the structure make it improbable that missionary zeal was Abelard's reason for writing.[31]

The overall impression gained from these works is not merely that they fail to explicate a missionary intention. If the absence of proselytism were the only common feature, one might assume that the motive to convert was so integral to polemic that the authors took it for granted. Instead, work after work presents ideological reservations about mission, reluctance to engage in debate, defensive

[27] Guibert de Nogent, *Tractatus de Incarnatione contra Judaeos*, *PL* 156: 489–90; and *De vita sua*, *PL* 156: 949–50.

[28] The work of "Samuel of Morocco" may well be a pseudepigraph of the fourteenth century. See M. Marsmann, *Die Epistel des Rabbi Samuel an Rabbi Isaak: Untersuchung und Edition* (Siegen, 1971). The epistle also appears in *PL* 149: 337–68.

[29] Petrus Alfonsi, *Dialogus Petri, cognomento Alphonsi, ex Judaeo Christiani, et Moysi Judaei*, *PL* 157: 535–672, esp. 538.

[30] Niemeyer, *Hermannus quondam Judaeus opusculum de conversione sua.*

[31] Peter Abelard, *Dialogus inter philosophum, iudaeum, et christianum*. ed. Rudolf Thomas (Stuttgart, 1970), 44–85. Richard of St. Victor's *De Emmanuele* was directed against Andrew of St. Victor's Judaizing commentary on Isa. 7:14 and is surely not a missionary tract. See *PL* 196: 601–66.

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explanations for writing polemical works, and justifications based on the need to combat heresy and to instruct a Christian audience—all of which point to a striking lack of interest in a missionary program. Either mission was a secondary motive or not a motive at all or else these authors felt uncomfortable asserting it. In either case, the ideology they expressed—at the very minimum—attached little importance to conversion of Jews.

There are, however, three twelfth-century works that contain signs of things to come. The first, emerging from the school of Abelard, is known for its striking use of Hebrew as a tool in the debate with Jews, but, in light of the objectives of earlier polemics, the motive it suggests for disputation is at least equally interesting. Sometime between 1139 and 1148, an obscure cleric named Odo wrote the *Ysagoge in theologiam*. In the introduction to the section on Jews, he made the following assertion: "For if it is proper for us to exhort those who are fashioned in the faith to live better, surely we should recall the Jews from their erroneous, disbelieving sect."[32] If such an attitude were common, this would have been an utterly routine sentence. The editor of the *Ysagoge*, for example, wrote that "the conversion of the Jews was one of the great preoccupations of Christian intellectuals in the twelfth and thirteenth centuries," and Odo was a participant in this movement.[33] In fact, a statement like this before the middle of the twelfth century was not routine but sharply polemical; it was a pointed *a fortiori* argument directed against the then-dominant view of the upper clergy that efforts at conversion of Jews were improper or unimportant.

The later attitude of aggressive mission to the Jews is adumbrated with particular clarity by Peter the Venerable. The reader of the prologue to his polemic finds himself in a different, unfamiliar world. No hesitation here, and no apology. How can Jews, he wrote, alone in all the world, deny Jesus? They are stiff-necked, without celestial or terrestrial glory, but, if they convert, they, too, can be saved.[34] Later in the work, Peter expressed doubts about his prospects for success. With the arrogance and belligerence typical of this polemic, he noted that his arguments from both authority and reason would satisfy any human being, but he was not so sure that Jews, whose reason appeared "extinct" and "buried," could be called human beings and not animals.[35] Whatever the tactical wisdom of his denunciatory tone, and whether or not Peter ever had contact with his prospective converts, there is in his work at least some expression of a hope of conversion.[36]

Finally, later in the century, we come to an author whom we might well expect to find in any list of exceptional figures: Joachim of Fiore. Nevertheless, not all of his discourse is exceptional. Joachim began with the familiar assertion that response to the Jews is necessary because otherwise one gives occasion to the

[32] *Ysagoge in theologiam*, ed. Arthur M. Landgraf, *Ecrits théologiques de l'école d'Abélard* (Louvain, 1934), 126–27. Also see Avrom Saltman, "Ha-Ysagoge shel Odo—Shitah Ḥadashah Ba-Pulmus Ha-Anti-Yehudi," *Biqqoret U-Parshanut*, 13–14 (1979): 265–80.

[33] *Ecrits théologiques*, xlvii.

[34] Peter the Venerable, *Tractatus adversus Judaeorum inveteratam duritiem*, *PL* 189: 507–09.

[35] *Ibid.*, 602.

[36] It may be more than coincidence that both Odo and Peter, who were interested in genuine mission, used tools borrowed from the Jewish armory—in the first case linguistic and in the second talmudic.

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enemies of Christ to insult the faith and confuse the simple believer. He went on, however, to a consideration peculiar to his own well-known speculations about the imminence of a new age. An additional reason for the work, he said, was his feeling that the Jews would soon experience the divine mercy as the time of their consolation and conversion arrived.[37] At that time, all Jews would convert. Joachim, however, wanted some to see the light just before the period of general salvation, and he broke into prophetic exhortation: "And now, O Jewish men, hear my voice this day, and do not persist in hardening your heart."[38]

These exceptions are few in number, and there is less to them than meets the eye. The relevant section of the *Ysagoge in theologiam* is a manifestly atypical work by an insignificant author, and Joachim of Fiore is a profoundly idiosyncratic figure whose position on Jewish conversion flows precisely from his most important idiosyncracy. As for Peter the Venerable, his bitter pessimism about the prospects of persuading the Jews drastically tempers the impression of missionary zeal that his remarks may create, and the *Tractatus* remains far more a work of denunciation than of mission.

In the thirteenth century, sentiments for proselytism continued to grow, and ultimately they prevailed. Peter of Cornwall's disputation, completed in 1208, describes at great length his successful effort to convert a Jewish acquaintance, although, as in the reworking of Crispin's polemic, the object of this effort is a single individual.[39] In the 1230s, William of Bourges wrote that, shortly after his conversion to Christianity, he was urged to use his knowledge of Hebrew to compose a work to refute the Jews. After all, Jesus himself fought against both Sadducee heretics and other Jews, and, if Christians truly love him, they should do battle against his enemies. Ominously, William's proof text is "Shall I not hate those who hate you, O Lord?" (Ps. 139:21).[40] Whether the motivation was hate or love, by the mid-thirteenth century a Christian campaign to convert the Jews was gathering momentum,[41] and the theoretical desirability of such a program was not again seriously questioned until modern times.

THE POLEMICAL WORKS THAT I HAVE EXAMINED do more than reveal the absence of a missionary ideology; they also make assertions about frequent discussions between Jews and Christians at and especially below the level of the upper clergy. Such information, if authentic, is of extraordinary historical value. Assessing authenticity is, of course, no easy task. We are dealing in many of these instances with a literary genre of fictitious debate, which led one scholar to regard virtually

[37] Joachim of Fiore, *Adversus Judaeos di Gioacchino da Fiore*, ed. Arsenio Frugoni (Rome, 1957), 3.

[38] *Ibid.*, 85–89. The points in this paragraph were made by Frugoni in the introduction to his edition (pp. xxxii–xxxvii). I hesitate to include Hildebert of Lavardin's short sermon "Against the Jews Concerning the Incarnation" among these exceptional polemics, despite its apostrophe to the Jews urging their conversion. The entire work is a few paragraphs long, was delivered to a Christian audience, and merely lists a handful of the standard verses on the incarnation with virtually no argumentation. See *PL* 171: 811–14.

[39] The prologue to Peter's *Liber disputationum contra Symonem Iudeum* was published by Hunt; "Disputation of Peter of Cornwall," 153–56.

[40] Guillaume de Bourges, *Livre des guerres du Seigneur*, ed. Gilbert Dahan (Paris, 1981), 66, 68.

[41] Cohen, *The Friars and the Jews*.

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all of the major polemics besides Crispin's as possessing "no historical interest."[42] There are, however, ways of evaluating this evidence.

First, the requests for polemical material were genuine. It would require a perverse level of skepticism to assume that Adam of Perseigne invented a request so that he could explain why he turned it down. Peter Damian's entire personal history and psychology indicate that he was sincere in asserting that one should concentrate on battling the vices of the flesh and that his reluctant agreement to enter the lists against the Jews resulted from a letter of request.[43] In Rupert's case, a somewhat later request from his correspondent exists. One cannot be certain about Peter of Blois, but the evidence in the other cases places the burden of proof on the skeptic. It appears that the lower clergy, precisely because of greater contact with the outside world, felt a need for works that would assist them in the religious discussions that were apparently a common feature of everyday life.

The evidence, moreover, does not allow the assumption that these discussions were necessarily initiated by proselytizing Christians. The assertions of Jewish aggressiveness in the works of Peter of Blois and Walter of Châtillon and in the anonymous *Tractatus* may be exaggerated, but they would constitute silly, almost self-defeating bombast if they did not have some basis in reality.[44] Furthermore, the testimony in these polemics is borne out to a striking extent by thirteenth-century Jewish works. Whether Jews or Christians initiated these exchanges, the indications are overwhelming that they were real and frequent. The nature of some of the arguments as well as circumstantial evidence support this conclusion.

The most detailed account of a Jewish-Christian debate in a Christian work is that of Herman of Cologne. Here a Jewish youth in early twelfth-century Germany listens to a Christian sermon that describes Jews as animals who understand only the letter of the law, in contrast to Christians, who are human beings who use reason to understand the spirit of the law.[45] He is directly exhorted to give up the heavy yoke of the Mosaic law and take up instead the easy burden offered by Jesus (Matt. 11:30),[46] and at one point he initiates a conversation with no less a figure than Rupert of Deutz himself.[47] Local churchmen provide him with books, and he maintains that he succeeded in teaching himself Latin so that he could read

[42] Lévi, "Controverse," 239.

[43] See my discussion in "St. Peter Damian," 83.

[44] The remarks by Peter and the author of the *Tractatus* were noted by Lévi, and many historians have cited Louis IX's comment that a Christian layman approached by a Jewish polemicist should respond by stabbing him. See Lévi, "Controverse," 238. Also see my brief discussion in *The Jewish-Christian Debate*, 22–23. Compare the somewhat weaker impression given by Peter Damian that Honestus was confronted by a Jewish challenge, and see Bartholomew of Exeter's comments cited in note 18, above. Guibert's remark that the Jews hardly dared whisper what the count of Soissons said aloud does reflect some Jewish caution, but it must also be read in light of Guibert's strategy to use the Jews as a foil for the heretical count.

[45] Niemeyer, *Hermannus quondam Judaeus opusculum de conversione sua*, 74. This image appears in the work of Walter of Châtillon and, more clearly, in the polemic of Peter the Venerable. Also see my "The Attitude of St. Bernard of Clairvaux toward the Jews," *Proceedings of the American Academy for Jewish Research*, 40 (1972): 103.

[46] Niemeyer, *Hermannus quondam Judaeus opusculum de conversione sua*, 75. Such an argument by Christian missionaries may have partially inspired and surely lent force to the Jewish contention that conversion to Christianity proved nothing more than the convert's desire to experience the pleasures of the flesh; *Niẓẓaḥon vetus*, in *The Jewish-Christian Debate*, 206, and, in the Hebrew section, 144.

[47] Niemeyer, *Hermannus quondam Judaeus opusculum de conversione sua*, 77–83.

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them.[48] Nevertheless, he insists that what really clinched his decision to convert was his observation of the prayerful devotion of simple nuns.[49] The strong impression that emerges from his work is not that of an intellectual poring over sophisticated tracts or even disputing with people like Rupert; rather, we see a Jew who maintains regular, intimate contacts with ordinary Christians and lower clergy and who is eventually won over to the dominant religion by an accumulation of such experiences. Although Herman reported that a Jew chastised him for excessive association with Christians,[50] the impression of frequent religious discussions between ordinary Jews and Christians is by no means negated by the undeniable fact that Herman went too far. The atmosphere of the memoir is compelling.

The reality of such contacts emerges from a number of other Christian works as well. Rudolph of St. Trond, the abbot who requested Rupert of Deutz to write his polemic, is reported to have "frequently held mild discussions with Jews without disputation or reproach; rather, he softened the hardness of their heart by stroking and massage. . . . For this reason they loved him so much that even their women came to see him and speak with him."[51] The general tone and content of Crispin's disputation has convinced most scholars of its essential authenticity as a work arising out of friendly, informal meetings between the author and a Jewish acquaintance.[52] Odo of Cambrai's assertion that he had an unplanned discussion with a Jew is highly plausible because he has another explicit motive for writing his work: the addressee, as we recall, had requested that he record his lecture on the incarnation.

Moreover, Odo's polemic ends with a fascinating sentence that suggests not only the reality of such discussions but also their context. "These . . . are the reasons that I gave the Jew concerning the coming of Christ, having been forced to dispute all the more subtly by certain Christians who took the part of the Jew."[53] Thus, as in Crispin's case, there was an audience, and here some Christians attending were prepared to challenge the arguments of the Christian protagonist. Even if these Christians were advocates of an explanation of the incarnation that differed from Odo's, such intervention would be inconceivable in a debate whose serious goal was the conversion of the Jew. These confrontations were ultimately very serious indeed, but the atmosphere appears to have been one of a duel of wits—almost a form of intellectual entertainment.

[48] *Ibid.*, 76.
[49] *Ibid.*, 107–08.
[50] *Ibid.*, 93.
[51] *Gesta abbatum Trudonensium*, quoted by Niemeyer, in his introduction to *Hermannus quondam Judaeus opusculum de conversione sua*, 5 n. 3.
[52] In composing such a work, an author naturally expands and "improves" the discussion; hence, certain implausibilities in the exchange do not in themselves undermine the likelihood of an encounter, and even R. J. Zvi Werblowsky, who expressed serious reservations about the recorded disputation, did not doubt Crispin's statement that he held amicable discussions about religion with a Jewish acquaintance. See Werblowsky, "Crispin's Disputation," *Journal of Jewish Studies*, 11 (1960): 73. The reworking of Crispin presents a fictitious exchange, and I would therefore treat it more cautiously than did Aryeh Grabois, who said that the work "clearly attests" frequent, informal meetings among intellectuals. Nevertheless, the author's assertion that he had such discussions, even though it too is borrowed from Crispin, presumably reflects a milieu in which such a report would sound plausible. See Grabois, "The *Hebraica veritas*," 634.
[53] Odo of Cambrai, *Disputatio contra Judaeum*, *PL* 160: 1112.

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Guibert's polemic, which does not reflect a real confrontation, ends with a miracle story also pertinent to this discussion. He heard an account of a disputation in a home (*in quadam domo*) in which a cleric was unable to contest the perfidious bombast of a Jew, so the cleric offered to hold the burning part of a firebrand in order to prove his position. The Jew made no effort to dissuade him, and the cleric grabbed hold of the flame and did not burn. The Jew marveled but was nonetheless not impelled to convert.[54] The miracle here is not especially miraculous, and the story could be true. Even if it is not, however, it suggests that such discussions were routine.

Finally, both Peter of Blois and the author of the anonymous *Tractatus* proffered practical advice on pinning down the slippery and elusive Jewish disputant, who was likely to change the subject whenever he encountered difficulty.[55] Once again, works that do not record actual disputations suggest that Jews and Christians expected to confront one another in the field of religious combat.

Thus far, I have examined only Christian works, but the impression created by those works is confirmed by Jewish polemics as well. This literature does not begin until the late twelfth century, and one of the earliest works, authored by the southern French polemicist Jacob ben Reuben, reports an encounter whose essential historicity has never been questioned.[56] The tone is cordial, the arguments rigorous, and the agenda—which includes a discussion of the book of Matthew—unusually broad. In the thirteenth century, Meir of Narbonne recorded what were surely genuine exchanges with influential Christians on sensitive questions,[57] and Moses of Salerno described philosophical discussions of unusual sophistication with Italian churchmen.[58] These were not formal disputations of the sort that were held in Paris and Barcelona; Jacob, Meir, and Moses described what were for the most part informal discussions that took place in the course of everyday life.[59]

Finally, there is fascinating and somewhat problematical evidence from northern Ashkenaz in Joseph Official's *Sefer Yosef HaMeqanne*[60] and the anonymous *Niẓẓaḥon vetus*.[61] On the one hand, the sharpness of some of the exchanges in these

[54] Guibert de Nogent, *Tractatus de Incarnatione contra Judaeos*, *PL* 156: 528. For an eleventh-century proposal to prove Christianity through ordeal by fire to an audience of similarly unimpressed Muslims, see Kedar, *Crusade and Mission*, 45.

[55] Peter of Blois, *Contra perfidiam Judaeorum*, *PL* 207: 870; and *Tractatus adversus Judaeum*, *PL* 213: 749.

[56] *Milḥamot HaShem*, ed. Judah Rosenthal (Jerusalem, 1963), esp. xxii, 4–5.

[57] *Milḥemet Mizvah*, Biblioteca Palatina Parma, MS. 2749. A substantial part of the manuscript was transcribed by William Herskowitz. See Herskowitz, "Judaeo-Christian Disputation in Provence as Reflected in Milḥemet Mitzva of R. Meir HaMeili" (D.H.L. dissertation, Bernard Revel Graduate School, Yeshiva University, 1974). On Meir's work, see Siegfried Stein, *Jewish-Christian Disputations in Thirteenth-Century Narbonne* (London, 1969). Also see the studies by Robert Chazan in *Harvard Theological Review*, 67 (1974): 437–57; *Hebrew Union College Annual*, 45 (1974): 287–305; and *Proceedings of the American Academy for Jewish Research*, 41–42 (1973–74): 45–67.

[58] *Ta'anot*, in Stanislaus Simon, *Mose ben Salomo von Salerno und seine philosophischen Auseinandersetzung mit den Lehren des Christentums* (Breslau, 1931).

[59] It is worth noting that in the Barcelona disputation of 1263, Naḥmanides commented that "there is not a single priest or child" who does not ask the Jews about Ps. 110. See *Kitvei Ramban*, ed. C. Chavel (Jerusalem, 1963), vol. 1, p. 317.

[60] Joseph Official, *Sefer Yosef HaMeqanne*, ed. Judah Rosenthal (Jerusalem, 1970).

[61] See *The Jewish-Christian Debate*. The *Niẓẓaḥon vetus* was also edited by Mordechai Breuer; *Sefer Niẓẓaḥon Yashan* (Jerusalem, 1978).

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works invites skepticism about their authenticity. Once again, however, the atmosphere of constant interaction is compelling, and it is almost inconceivable that these accounts are not essentially authentic. Most of the arguments are introduced by phrases like "a certain *cordelier*" or "a certain apostate asked." Specific priests are identified by their towns, and arguments are placed in specific settings.

Moreover, the aggressiveness of the tone of both works makes it difficult to reject Christian assertions that Jews often initiated debate. It is true that one of the most distinguished students of this literature has urged us to differentiate between "audacity in confronting Christianity" and the initiation of disputation,[62] and in some instances this is a useful caveat. But the assertiveness of the Ashkenazic polemics must undercut skepticism about the validity of Christian reports concerning Jewish initiatives. Jews who urged their readers to tell Christians that Jacob sat on a cross,[63] who reported (falsely or not) that a Jew urinated on a cross in the presence of a churchman and then produced a clever justification,[64] who clearly suggested to their readers that they raise embarrassing questions with Christians[65]—Jews who said such things and more cannot be assumed a priori to have shrunk from initiating religious discussions with Christian acquaintances. Even if the authors—despite the plain meaning of their exhortations—expected discretion from their Jewish readers, all readers would not have obliged. In short, the existence of such polemics practically guarantees that Jews who took them literally would act on their advice, and, while the worst excesses of these works may never have been translated into practice, it is hard to deny that a number of readers would have been impelled to challenge Christians to defend their faith. There were, of course, cautious Jews,[66] but bold, even reckless, disputants, especially in northern France and Germany, appear to have constituted far more than a lunatic fringe.

Even Jewish familiarity with Christian books often resulted from these discussions since the access of Jews to such works normally came through Christians who owned them. Herman of Cologne was given Latin books, and Jacob ben Reuben said that his Christian friend gave him a work that apparently was—at least in part—a polemical anthology.[67] Although sections of some Jewish polemics appear to have been composed to refute written Christian exegesis,[68] most of the points were debated in lively and frequent discussions.

[62] Frank Talmage, *Commentary*, June 1975, p. 23.

[63] This is a delicate paraphrase of the original. See *Nizzaḥon vetus*, in *The Jewish-Christian Debate*, 59, and, in the Hebrew section, 20.

[64] Joseph Official, *Sefer Yosef HaMeqanne*, 14. There is some ambiguity in the story as to whether the Jew was aware that the Christian would see him.

[65] See *The Jewish-Christian Debate*, sects. 156, 161, 188, 206, 210, 229.

[66] The most striking example of a cautious polemicist is Solomon de' Rossi. See Frank Talmage, "Christianity and the Jewish People," in his *Disputation and Dialogue* (New York, 1975), 240. Here Solomon's position is presented as more or less typical. Also see *The Jewish-Christian Debate*, 21–22, and n. 55. In addition, it should be kept in mind that the greatest figures of medieval Jewry, at least in the period with which we are concerned, did not write polemical works. (Naḥmanides' coerced involvement in Barcelona is, of course, not germane to the present discussion.)

[67] See my discussion in *Speculum*, 49 (1974): 36–37, 46–47.

[68] When Jewish works, for example, refute Christological interpretations that are found only in Christian commentaries and not in polemics, we have reason to suspect that the Jewish authors got the information from a literary source, and a systematic investigation along these lines may well prove rewarding. For a clear-cut passage of this sort, note the probably interpolated section in the Munich manuscript of the *Nizzaḥon vetus* on Psalms, with its explicit references to Christian translations and to

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It is difficult to characterize with precision the current state of scholarship on this question. Some skepticism remains about Jewish initiation of debate.[69] But scholars who have concentrated on the northern Ashkenazic polemics have found it difficult to discount an impression of authenticity, whatever the degree of their reservations on matters of detail,[70] and this impression is consistent with the general picture reflected in other Jewish sources from Northern Europe.[71] Students of Christian polemic in the High Middle Ages have remarked very briefly on the existence of a genuine Jewish challenge,[72] and here, too, the study of nonpolemical Christian sources confirms the impression of significant interaction.[73] My review of polemic—despite the limitations and inevitably skewed emphasis of the genre—underscores that debate was a central phenomenon in the social and intellectual life of medieval Ashkenazic Jewry. While evidence of extensive debate decidedly does not demonstrate that other developments in the cultural history of Ashkenazic Jews were influenced by Jewish-Christian contacts (particularly since the greatest rabbis were not polemicists), the instinctive resistance that many historians still feel when such assertions are made should be diminished.

One more important point. Jewish aggressiveness and even Jewish initiative do not constitute a Jewish mission. There is no indication that Jews engaged in religious discussions with Christians with the realistic expectation of converting

the *glossa* and its concentration on exegesis that no sensible Christian polemicist would have emphasized. See *The Jewish-Christian Debate*, sects. 131–41. The extent to which Jews could have read Latin works depends, of course, on their knowledge of Latin, and, although almost all of the authors of polemical works surely read Latin, we cannot be certain about other Jewish intellectuals. For an argument that the Paris disputation of 1240 was conducted in Latin, see Ch. Merchavia, *HaTalmud BiRe'i HaNaẓrut* (Jerusalem, 1970), 245. Grabois's assertion that "Rashi attested that he studied Christian biblical exegesis" is much too strong. Of the two authorities that Grabois noted, Y. Baer presented very little evidence for his assertion that "we may assume that Rashi knew Latin and read widely in Christian works," and E. Shereshevsky explicitly conceded that there is no definitive evidence that Rashi read Latin. See Grabois, "The *Hebraica veritas*," 632; Baer, "Rashi VeHaMeẓiut HaHistorit shel Zemanno," *Tarbiẓ*, 20 (1950): 326; and Shereshevsky, "Rashi and Christian Interpretations," *Jewish Quarterly Review*, 61 (1970–71): 76–86.

[69] Talmage, *Disputation and Dialogue*, 240; and *Commentary*, June 1975, p. 23.

[70] Zadoc Kahn, "Le livre de Joseph le Zélateur," *Revue des Etudes Juives*, 1 (1880): 222–46; and 3 (1881): 1–38, esp. 34; Ephraim Urbach, "Etudes sur la littérature polémique au moyen âge," *Revue des Etudes Juives*, 100 (1935): 50–77, esp. 60–64; and Mordechai Breuer, *Sefer Niẓẓaḥon Yashan*, 20–21. Also see my discussion in *The Jewish-Christian Debate*, 20–23.

[71] See, especially, Jacob Katz, *Exclusiveness and Tolerance* (New York, 1960), 98–100. Also see Grabois, "The *Hebraica veritas*." For a vigorous argument, based primarily on Jewish exegetical material, for Jewish-Christian intellectual contacts, see Elazar Touitou, "Shitato HaParshanit shel HaRashbam al Reqa HaMeẓiut HaHistorit shel Zemanno," in Y. D. Gilat *et al.*, eds., *Iyyunim BeSifrut Ḥazal BaMiqra U-veToldot Yisrael: Muqdash LiProf. Ezra Zion Melamed* (Ramat Gan, 1982), 48–74.

[72] Peter Browe assessed the situation particularly well. See Browe, *Die Judenmission im Mittelalter und die Päpste* (Rome, 1942), 113, 60–64. Also see Hunt, "Disputation of Peter of Cornwall," 147; and Dahan, *Livre des guerres du Seigneur*, 33–34. Blumenkranz's discussion of "la mission juive" deals mainly with an earlier period; *Juifs et Chrétiens*, 159–211. As I noted, Blumenkranz ascribed a significant missionary motivation to Christian polemic in his period. Elsewhere, however, he argued that the extent of what he described as Christian defense literature demonstrates that Jews must have pursued missionary activity; *Juifs et Chrétiens*, 209. In light of his position on the missionary objectives of Christian polemic, this last argument is almost puzzling, and the book, which remains of the first importance, tends to overstate the missionary intentions on both sides.

[73] See Grabois, "The *Hebraica veritas*." Polemical sources are dealt with only in the two concluding paragraphs; *ibid.*, 633–34, esp. 624.

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them. Both the Talmud and the status of medieval Jewry militated against such a program, and the occasional evidence of Christian converts to Judaism does not begin to demonstrate a concerted Jewish effort to attract proselytes.[74] Jews challenged Christians as an expression of pride—to raise their own morale and to discomfit their opponents. Joseph Official wrote "to reveal the shame" of Jewish apostates.[75] The author of the *Nizzaḥon vetus* completed his advice to polemicists by promising that "then you will find the Gentile thoroughly embarrassed; indeed, he will be found to have denied [Christianity's] central dogmas, while all Israel 'will speak lovely words' (Gen. 49:21)."[76] A chastened Gentile with an enhanced respect for Jews and Judaism—but a Gentile nonetheless.

THE ABSENCE OF A CHRISTIAN MISSIONARY ideology and the presence of frequent Jewish-Christian confrontations establish the likelihood that eleventh- and twelfth-century Christians wrote polemics not out of missionary objectives but largely in response to requests generated by a genuine Jewish challenge. There is, however, a more profound relationship between the disinterest of the upper clergy before the thirteenth century in converting Jews and the existence of lively, regular, often friendly debates between Jews and Christians, which were sometimes begun by the Jewish participant. The tone of these informal contacts and the Jewish willingness to initiate them were possible precisely because the church was not yet deadly serious about the aim of conversion. For Jews, the enjoyment was drained out of these contests in the face of a concerted, formalized Christian mission, and it became foolhardy and dangerous to seek confrontation. During the course of the thirteenth century, the gradual transformation in the Christian position was not immediately reflected in Jewish behavior, and, even later, the spirit never completely departed from Jewish polemicists. Nevertheless, by the late Middle Ages the tone is profoundly different; one begins to see the defensiveness, nervousness, and demoralization of a worried community. Jewish polemic was never the same again.[77]

[74] Wolfgang Giese's vehement argument for "intensive Jewish propaganda and missionary activity" is based solely on the existence of Christian converts to Judaism and the efforts made in church councils to limit contacts between Jews and Christians because of the Jews' corrupting influence. See his "In Iudaismum lapsus est: Jüdische Proselytenmacherei im frühen und hohen Mittelalter (600–1300)," *Historisches Jahrbuch*, 88 (1968): 407–18. Also see Baron's remark that "medieval Jews had long given up any missionary aspirations"; *A Social and Religious History*, 23.

[75] Joseph Official, *Sefer Yosef HaMeqanne*, 15. See Hos. 2:2.

[76] *The Jewish-Christian Debate*, 169, and, in the Hebrew section, 108.

[77] The changes in Jewish polemic do not, of course, result solely from increased Christian proselytizing and persecution. Most late medieval Jewish polemic comes from Spanish Jewry rather than from areas where Jewish aggressiveness is most clearly attested in the earlier period. For the decline in polemic and the growing isolationism in the Ashkenazic orbit in the fifteenth, sixteenth, and seventeenth centuries, see Katz, *Exclusiveness and Tolerance*, chaps. 11, 12. On Spain, see the striking personal testimony of an obscure participant in the Tortosa disputation; Frank Talmage, "Trauma at Tortosa: The Testimony of Abraham Rimoch," *Medieval Studies*, 47 (1985): 397. I am grateful to Moshe Idel for bringing this last reference to my attention.

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Scholarship and Intolerance in the Medieval Academy: The Study and Evaluation of Judaism in European Christendom

JEREMY COHEN

RECENT DEVELOPMENTS IN THE FIELD of literary criticism have emphasized the influence of texts and textual traditions in shaping the world views of individuals and the cultures of societies. Perceptions of meaning, structure, and authority in literature, as well as the extent of its accessibility, reflect and contribute to manifold components of a community's ethos, ranging from the political and socioeconomic to the religious and intellectual. It follows that new methods for studying particular texts, or any alteration in the prevailing level of familiarity with them, may ultimately affect existing patterns of interpersonal relations and hitherto regnant ideologies in a given culture. Viewed within the context of the rise of urban schools and universities in high medieval Europe, the orientation of Christian scholars to the biblical and rabbinic literature of the Jews offers a fascinating case in point. Christianity had always acknowledged its debt to the Old Testament,[1] but the medieval church could not forget that a community of unbelievers—a people unique in its ability to withstand Christianity's conquest of the Roman world and to secure legal standing in Western Christendom nonetheless—served as the guardian of the original Hebrew text of the Old Testament and maintained the most literal observance of its precepts. Modern scholars have long recognized that, as the focus of medieval intellectual creativity shifted from monastery to urban school, Christian concern for the *Hebraica veritas* and rabbinic exegesis of the Bible increased markedly. This process had several implications for scholarly method and religious ideology. As their interest in Hebraic texts rose, Christian scholars of the period came to espouse the view that these texts, to serve as a valid source of religious and historical knowledge, had to be measured against the beliefs and instruction of contemporary Jews. Yet when influential churchmen, true to the Scholastic spirit of their age, turned to these beliefs as a subject for academic scrutiny in their own right, they perceived that such a comparison threatened to

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[1] The term "Old Testament," befitting the theological perspective of the Christian subjects of this essay, has been used for convenience.

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undermine established notions of the relationship between the church and the synagogue. Ultimately, medieval theologians realized the need for a new approach to the Jews and their textual traditions, attuned to historical reality but compliant with the demands of fundamental Christian doctrine.

I believe that there is a linkage between the intellectual awakening of the twelfth and thirteenth centuries, on the one hand, and simultaneous changes in Christian anti-Judaism, on the other. Granted, this period witnessed important political and socioeconomic developments that directly influenced the way Europeans treated the Jews—along with changes in the Jewish posture toward Christians and their faith—but the present discussion will not concern them. Rather, three contemporaneous trends in high medieval culture contributed to an increase in Christian anti-Judaism: (1) methodological advances in Christian scholarship (ever present in this period) and the gradual emergence of theology as an independent academic discipline; (2) the Christian pursuit of Hebraic scholarship and literalist exegesis of the Old Testament; and (3) the increasingly hostile ideological stance of the church toward the European Jewish community. Each of these developments has received ample consideration in recent scholarly literature, but the nature of their interdependence still awaits explication.[2] Focusing on the nexus between method and ideology in the study of religious texts, I propose to assess the significance of that interdependence prior to the career of Anselm of Canterbury and the First Crusade, in the midst of the cultural ferment and experimentation that characterized the twelfth century, and through the high Scholastic attempts at intellectual synthesis in the thirteenth century.

DESPITE THE EXCEPTIONAL AND ADMITTEDLY significant portents of change in Christian exegesis that appear in some eleventh-century Christian polemics, the study of Christian doctrine before the twelfth century principally consisted of the reading and figurative interpretation of the Bible. Removed from the hubbub of

[2] The following works were of particular importance in laying the basis for this study. On Scholasticism and the academic study of theology, see Martin Grabmann, *Die Geschichte der scholastischen Methode*, 2 vols. (1909–11; reprint edn., Berlin, 1956); M.-D. Chenu, *La théologie au douzième siècle*, Etudes de philosophie médiévale, vol. 45 (Paris, 1957), and *La théologie comme science au XIII^e siècle*, Bibliothèque thomiste, vol. 33 (3d edn., Paris, 1957); Gordon Leff, *Medieval Thought: St. Augustine to Ockham* (Chicago, 1957); R. W. Southern, *Medieval Humanism and Other Studies* (Oxford, 1970); Jaroslav Pelikan, *The Growth of Medieval Theology (600–1300)*, The Christian Tradition, vol. 3 (Chicago, 1978); G. R. Evans, *Old Arts and New Theology: The Beginnings of Theology as an Academic Discipline* (Oxford, 1980); Robert L. Benson and Giles Constable, eds., *Renaissance and Renewal in the Twelfth Century* (Cambridge, Mass., 1982); and Stephen C. Ferruolo, *The Origins of the University: The Schools of Paris and Their Critics, 1100–1215* (Stanford, Calif., 1985). On Christian Hebraic scholarship and biblical exegesis, see Beryl Smalley, *The Study of the Bible in the Middle Ages* (3d edn., Oxford, 1983); Henri de Lubac, *Exégèse médiévale: Les quatre sens de l'écriture*, Théologie, vols. 41–42, 59 (Paris, 1959–64); Herman Hailperin, *Rashi and the Christian Scholars* (Pittsburgh, Pa., 1963); Aryeh Grabois, "The *Hebraica veritas* and Jewish-Christian Intellectual Relations in the Twelfth Century," *Speculum*, 50 (1975): 613–34; and G. R. Evans, *The Language and Logic of the Bible: The Earlier Middle Ages* (Cambridge, 1984). On anti-Jewish polemic, see Lena Dasberg, *Untersuchungen über die Entwertung des Judenstatus im 11. Jahrhundert* (Paris, 1965); Amos Funkenstein, "Ha-Temurot be-Vikkuaḥ ha-Dat she-bein Yehudim le-Notsrim ba-Me'ah ha-12" (Changes in the Patterns of Christian Anti-Jewish Polemic in the Twelfth Century), *Zion*, new ser., 33 (1968): 125–44; Ch. Merchavia, *Ha-Talmud bi-R'i ha-Natsrut: Ha-Yahas le-Sifrut Yisra'el shele-Aḥar ha-Miqra ba-'Olam ha-Notsri b-Imei ha-Beinayim [500–1248]* (The Church versus Talmudic and Midrashic Literature, 500–1248) (Jerusalem, 1970); and Jeremy Cohen, *The Friars and the Jews: The Evolution of Medieval Anti-Judaism* (Ithaca, N.Y., 1982).

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urban life, monastic scholars evidently did not feel the need—nor had they developed the means—to approach theology as a science in itself. They perpetuated the Augustinian notion that religious truth could be derived entirely from the text of Scripture, if only that text were properly understood.

This idea affected the Christian attitude toward the books of the Jews. Above all, it insured the centrality of the Old Testament—and, by extension, the opposition between the Old Testament and the New—in the study of theology. It also militated against the maturation of biblical exegesis as an academic discipline just as it impeded that of theology. If the Bible had to meet all of the theologian's needs, those needs a priori colored his appreciation of the scriptural text. According to Augustine, the perfection and goal (*plenitudo et finis*) of the Bible and biblical study were the love of God. Such a consideration not only relegated non-canonical texts to a markedly inferior status but also determined the appropriateness of literal or figurative exegesis for any specific passage of Scripture. Augustine explained simply to his readers, "Whatever in the word of God cannot be related in its literal sense to moral virtue or doctrinal truth you may recognize as figurative." As a result, Heiko Obermann correctly observed, there was "no objective standard by which to separate the literal sense—or the clear places in Scripture—from the allegorical sense—or the obscure places." This distinction was not grounded in the text but was "largely dependent on the relation of the reader to God."[3] No wonder that the historical study of Scripture was largely neglected during the early Middle Ages or that churchmen generally failed to procure the tools that would have promoted it—in the case of the Old Testament, familiarity with the Jewish traditions that embodied it and knowledge of Hebrew. Instead, because of their preoccupation with the contrast between church and synagogue, Catholic theologians identified the literal meaning of Mosaic law with all that the Christian covenant of grace had supplanted. In that case, the interest of pedant or antiquarian aside, how could the literal sense of the Old Testament enhance appreciation of Catholic doctrine? Therefore, if many publicly advocated the study of Hebrew for the sake of understanding the Bible, few bothered to act on their own advice.[4]

Alongside this valuation of the Old Testament developed a particular Christian attitude toward the medieval Jew who followed its precepts. Augustine had also instructed that the Jew had survived—and should survive—in Christendom, precisely because he maintained the biblical tradition that bespoke the truth of Christianity.[5] And, if doctrinal considerations shaped Christian understanding of Jewish Scripture, so too did they mold the Christian conception of the Jews, an idea

[3] Augustine, *De doctrina christiana*, 1.84–85, 3.33–34, CSEL 80: 30, 62–63, 88; and Heiko Augustinus Obermann, *Forerunners of the Reformation: The Shape of Late Medieval Thought* (New York, 1966), 283–84.

[4] On Christians' knowledge of Hebrew, see Berthold Altaner, "Zur Kenntnis des Hebräischen im Mittelalter," *Biblische Zeitschrift*, 21 (1938): 288–308; and Matthias Thiel, *Grundlagen und Gestalt der Hebräischkenntnisse des früheren Mittelalters*, Biblioteca degli studi medievali, vol. 4 (Spoleto, 1973).

[5] Augustine, *Tractatus adversus Judaeos*, *PL* 42: 51–67; *De civitate Die*, 18.46, 20.29, *Sermo*, 200.2, *PL* 38: 1030; *Enarrationes in Psalmos*, 58.1.21–22, CCSL 39: 744; and *De fide rerum invisibilum*, 6.9, CCSL 46: 15–16. Also see Bernhard Blumenkranz, *Die Judenpredigt Augustins: Ein Beitrag zur Geschichte der jüdisch-christlichen Beziehungen in den ersten Jahrhunderten* (1946; reprint edn., Paris, 1973), and "Augustin et les Juifs; Augustin et le judaïsme," *Recherches augustiniennes*, 1 (1958): 225–41.

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similarly out of touch with historical reality. Beryl Smalley aptly noted "how deeply the spiritual interpretation will penetrate language, thought, politics, and finally everyday life. The types are so real and so familiar that they may be used as arguments from authority, as well as for illustration. Their influence may be beneficent or sinister. The Synagogue is always opposed to the Church as darkness to light, and gradually this type becomes identified with the living Jew. . . . Philo's 'wise architect' [of allegorical exegesis] had built a prison for the Jewish people."[6]

The *Adversus Judaeos* tradition in patristic and medieval theology, through which the church asserted its legitimacy and validity, was serviced by the existence of Jews who typified life under the Old Testament. Doctrinally, the Jew had to remain, in Augustine's words, "stationary in useless antiquity," thereby bearing unconscious witness to the authenticity of the new Israel. This belief at the root of classical Christian theology helped insure the toleration of the Jews in early medieval Europe, but it prolonged Christian ignorance of the postbiblical, rabbinic religion that characterized medieval Jewish life, despite the legacy of classical rabbinic midrash that pervaded the methods and structures of patristic exegesis.[7]

Anti-Jewish polemicists of the early Middle Ages accordingly took little notice of the contemporary Jew, not because he was inaccessible to them but because, his proximity notwithstanding, he served little purpose in their arguments. These generally constituted an annotated list of biblical passages, usually from the Old Testament, which attested to the basic doctrines of Christian theology and which might depict the triumph of the church and the present subjugation of the Jews as foretold in Israelite prophecy.[8] Fulbert of Chartres, for example, whose three polemical sermons against the Jews undertook to prove their messianic hopes wrong and the doctrines of Christianity correct, relied entirely on biblical evidence. Like most medieval authors before him, Fulbert did not intend his polemic for use against a live Jewish audience but presented Jewish arguments—readily available in earlier Christian literature—as a foil, in order to strengthen the faith of his Christian listeners.[9] And, as late as the mid-eleventh century, Peter Damian, himself an avid exponent of reform in the Western Church, authored two anti-Jewish treatises that similarly rehashed the usual scriptural testimonies and

[6] Smalley, *Study of the Bible*, 25–26.

[7] On the influence of rabbinic midrash on patristic exegesis, see, among others, Harry Austryn Wolfson, *The Philosophy of the Church Fathers* (2d edn., Cambridge, Mass., 1964), 24–96 *passim*; and Raphael Loewe, "The Jewish Midrashim and Patristic and Scholastic Exegesis of the Bible," *Studia patristica*, 1 (1957): 492–524.

[8] Amos Funkenstein, "Basic Types of Christian Anti-Jewish Polemics in the Later Middle Ages," *Viator*, 2 (1971): 373–75. On early medieval anti-Jewish polemic, see A. Lukyn Williams, *Adversus Judaeos: A Bird's-Eye View of Christian Apologiae until the Renaissance* (Cambridge, 1935), bks. 4–5; Bernhard Blumenkranz, *Les auteurs chrétiens latins du moyen âge sur les Juifs et le judaïsme* (Paris, 1963); and Heinz Schreckenberg, *Die christlichen Adversus-Judaeos-Texte und ihr literarisches und historisches Umfeld (1.–11. Jh.)*, Europäische Hochschulschriften ser. 23, vol. 172 (Frankfurt am Main, 1982). R. W. Southern maintained that early medieval Spanish Christian scholars adopted a similarly affected ignorance toward Islam. Southern, *Western Views of Islam in the Middle Ages* (Cambridge, Mass., 1962), 25.

[9] Fulbert of Chartres, *Tractatus contra Judaeos*, *PL* 141: 305–18. Also see Blumenkranz, *Les auteurs chrétiens latins*, 237–43. On the availability of Jewish interpretations of Gen. 49:10 (the main text of Fulbert's sermons) in earlier Christian writings, see Adolf Posnanski, *Schiloh: Ein Beitrag zur Geschichte der Messiaslehre* (Leipzig, 1904), chaps. 4, 14.

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their established patristic interpretations. Of note are Damian's naive confidence that his biblical citations would triumph—in an actual face-to-face debate—over "every insanity of Jewish depravity" and his telling remark that disputing with contemporary Jews should not rank high among the priorities of the church.[10]

This relatively stagnant approach to the study of Jewish texts underwent dramatic change in the twelfth century. Lawyers, exegetes, and philosophers recognized more clearly the problem of conflicting authorities in the heritage of the church and offered a variety of solutions for reconciling them. Systems of rational analysis became the principal methodology in the exposition of sacred doctrine, challenging the procedure and substance of much patristic teaching based solely on the figurative interpretation of Scripture. The Crusades, which also reawakened Europe to the problematic status of the Jew, generally broadened the social, economic, and cultural frontiers of European Christendom. Cities, industry, and commerce were all on the rise and with them a new urban locus for the study of theology, a setting that inevitably increased the interaction between Christian scholar and Jew. Those who cultivated Christian spirituality no longer confined themselves to the monastery but turned directly to the natural world, seeking to understand it, to rationalize it, and to incorporate all of its facets into the totality of Christian life. The rebellion against patristic methodology, the insistence on a more scientific explication of texts, the demand that religious ideas bespeak the *realia* of history—all of these contributed to a renewed interest in and the greater influence of Jewish books in twelfth-century Christendom. With instructively different evaluations of the phenomena, Beryl Smalley, Henri de Lubac, M. D. Chenu, and others have all demonstrated the burgeoning of literalist exegesis of the Old Testament, the widening tendency to view the observance of Mosaic law as meritorious and justificatory in its own time, and the greater use of Mosaic precepts to explain and authenticate contemporary Christian institutions and practices. Concern for the *Hebraica veritas* gained prominence in Christian Old Testament commentary, and exegetes began to involve themselves in the study of the Hebrew language, either with the help of converted Jews or with that of medieval rabbis, who were themselves placing greater emphasis on the literal meaning of Scripture (*peshat*) rather than on its homiletic sense (*derash*). Yet Christian commentators sought far more than linguistic instruction from the Jews. For who could better understand the primary meaning and significance of the Old Testament than those who continued to observe and promulgate the literal sense of its teachings? Beryl Smalley put it well: "When the medieval scholar talked to a Rabbi he felt that he was telephoning to the Old Testament."[11] Literal-minded

[10] Peter Damian, *Antilogus contra Judaeos*, *PL* 145: 41: "Si Christi miles esse, et pro eo viriliter pugnare desideras, contra carnis vitia, contra diaboli machinas insignis bellator arma potius corripe; hostes videlicet, qui nunquam moriuntur: quam contra Judaeos, qui jam de terra pene deleti sunt." For Damian's *Antilogus*, see *PL* 145: 41–58. For a companion work, see *Dialogus inter Judeum requirentem, et Christianum, e contrario respondentem*, *PL* 145: 57–68. On both *opuscula*, see David Berger, "St. Peter Damian: His Attitude toward the Jews and the Old Testament, *Yavneh Review*, 4 (1965): 80–112; and Blumenkranz, *Les auteurs chrétiens latins*, 265–71.

[11] Beryl Smalley, *Hebrew Scholarship among Christians in XIIIth Century England as Illustrated by Some Hebrew-Latin Psalters*, Lectiones in Veteri Testamento et in rebus judaicis, vol. 6 (London, 1939), 1.

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commentators borrowed extensively from rabbinic scholars and from rabbinic literature, with the paradoxical result that in the realm of Christian exegesis evidence gleaned from contemporary Jewish sources frequently was deemed authoritative prima facie. The forthrightness and ease with which Hugh of St. Victor, his student Andrew, Peter Comestor, and Stephen Harding (to list only a few) justified their instruction from rabbis, juxtaposed rabbinic traditions with traditional Christian exegesis, and even preferred the former over the latter has been much discussed in recent scholarship, but it still cannot fail to astound the student of Christian-Jewish relations.[12]

One would expect that the perception of the medieval Jew as spokesman for the Old Testament would have reinforced the anti-Jewish tone of earlier polemics, grounded as they were in the premises that the biblical character of Judaism had not changed since antiquity and that the Jews therefore served to convey the meaning of their Scriptures to the Christians who tolerated them. Most of the extant Christian polemics from the twelfth century do validate this assumption. Some, to be sure, incorporate the conciliatory, rationalistic approach of Anselm of Canterbury, who asserted that the Jews could have rejected Jesus and Christianity only out of blind ignorance, which actually lessened their guilt for the crime of deicide; as rational human beings, the Jews needed only effective enlightenment to the truth.[13] Others, such as the popular and influential *Disputatio Judei et Christiani* of Gilbert Crispin, also manifest the rising frequency of intellectual exchanges between Christians and Jews, as well as a growing familiarity with Jewish responses to the Christological teachings of the church; the author of a theological treatise of the school of Abelard actually quoted Scripture in Hebrew as he appealed to the Jews to convert.[14] Nevertheless, as a general rule, the Jews continued to be typed as theological relics, the proponents of a carnal understanding of scriptural prophecy, which Christianity had supplanted and replaced. The language and arguments of the polemics remain overwhelmingly biblical. Polemicists failed to comprehend the historical significance and interpretative character of the evolving rabbinic tradition that regulated contemporary Jewish

[12] Smalley herself wrote that what "drove scholars to study the literal sense of the Old Testament" is "one of the darkest problems connected with the twelfth-century revival"; Smalley, "An Early Twelfth-Century Commentator on the Literal Sense of Leviticus," *Recherches de théologie ancienne et médiévale*, 35 (1969): 98.

[13] For example, see Anselm, *Cur Deus homo*, 2.15. Also see R. W. Southern, *Saint Anselm and His Biographer: A Study of Monastic Life and Thought, 1059–c. 1130* (Cambridge, 1963), chap. 3, and "St. Anselm and Gilbert Crispin, Abbot of Westminster," *Medieval and Renaissance Studies*, 3 (1957): 78–115. For a work at least worthy of mention in connection with this genre of rationalist polemic, see Peter Abelard, *Dialogus inter philosophum, Judaeum et Christianum*, ed. Rudolf Thomas (Stuttgart, 1970).

[14] See Crispin, *Disputatio Judei et Christiani*, ed. Bernhard Blumenkranz, Stromata, vol. 3 (Utrecht, 1956); and *Ysagoge in theologiam*, in *Ecrits théologiques de l'école d'Abélard: Textes inédits*, ed. Arthur Landgraf, Spicilegium sacrum lovaniense: Etudes et documents, vol. 14 (Louvain, 1934), esp. 126–62. On Crispin, see R. J. Z. Werblowsky, "Crispin's Disputation," *Journal of Jewish Studies*, 11 (1960): 69–79; and Anna Sapir Abulafia, "An Attempt by Gilbert Crispin, Abbot of Westminster, at Rational Argument in the Jewish-Christian Debate," *Studia monastica*, 26 (1984): 55–75. On the widespread popularity of the polemic, see Blumenkranz, "La *Disputatio Judei cum Christiano* de Gilbert Crispin, abbé Westminster," *Revue du moyen âge latin*, 4 (1948): 237–52; and David Berger, "Gilbert Crispin, Alan of Lille, and Jacob ben Reuben: A Study in the Transmission of Medieval Polemic," *Speculum*, 49 (1974): 34–47.

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life and that had emerged only after the birth of Christianity. As a result, they showed little interest in undermining the status of the Jew within Christian society, either through active proselytism or through forms of physical persecution.[15]

Even Peter Alfonsi's *Dialogue* with Moses the Jew (Peter's name before he converted from Judaism to Christianity), noted for its reference to a number of postbiblical talmudic homilies, conforms to this general pattern.[16] Peter stated at the outset that he composed his polemic in order to defend his recent conversion against Jewish charges of sacrilege, intellectual error, and opportunism. He never challenged the propriety of Jewish survival in Christendom, and he pursued his dialogue not with other prospective converts but solely with his alterego Moses. The debate ultimately concerns the correct interpretation of Scripture. Peter openly acknowledged that the Jews were the followers (*cultores*) of Mosaic law—their misinterpretation of it notwithstanding—and he acceded to Moses' request that in citing the Bible he make use of the *Hebraica veritas*. He devoted eight of the twelve chapters of his polemic to a justification of Christianity on primarily scriptural grounds. Only in the first three chapters did Peter turn to rabbinic texts to demonstrate his disenchantment with Judaism. "Show me," demanded Moses, "how it seems to you that the Jews have erred in the interpretation of the [Old Testament] Law, and that you indeed understand better." Pointing to the frequently anthropomorphic midrashim, a series of which he proceeded to criticize, Peter responded: "Inasmuch as you see them attending solely to the superficial meaning of the Law, expounding the text not according to the spirit (*spiritualiter*) but according to the flesh (*carnaliter*), whence they are deceived by the greatest error." Yet, while alleging that continued rabbinic adherence to the literal meaning of the Old Testament drove him away from the Jewish community, Peter actually cited an array of figurative, imaginative homilies that ranged far and wide beyond the letter of the biblical text.[17] Here lies the crux of the issue. From Peter's Christian perspective, any Jewish interpretation of the Old Testament was necessarily literal and thus to be distinguished from the true, spiritual, Christological reading of Scripture, for such was the role of the Jews in the divine economy of salvation. Although his Jewish upbringing and education should have enabled Peter to reveal the postbiblical character of contemporary Judaism to his Christian coreligionists, the demands of Augustinian theology weighed so heavily in his reading of Jewish texts that he could not. In his assessment of Jewish Scripture, Peter Alfonsi anticipated the judgment with which Peter of Blois

[15] Berger, "Gilbert Crispin"; Margaret Schlauch, "The Allegory of the Church and the Synagogue," *Speculum*, 14 (1939): 448–64; and M.-H. Vicaire,"'Contra Judaeos' meridionaux au début du XIII[e] siècle: Alain de Lille, Evrard de Béthune, Guillaume de Bourges," in *Juifs et judaïsme de Languedoc*, Cahiers de Fanjeaux, vol. 13 (Toulouse, 1977), 269–93.

[16] Peter Alfonsi, *Dialogus Petri et Moysi Judei*, *PL* 157: 535–672.

[17] *Ibid.*, 540. Subsequently, Peter wrote that, because the statements of the biblical prophets were often obscure, they should be interpreted allegorically if their literal sense would cause readers to veer from the path of reason ("talia . . . quae secundum litteram accipientes a rationis tramite exorbitemus, ea allegorice interpretamur"). The rabbis, however, did not perceive God properly ("non cognoverunt, ut oportuit, Deum"), and they therefore erred by interpreting prophetic teaching literally. Precisely in this regard Peter believed that he understood Scripture better than the Jews—that is, as reason would demand ("prout sanus exigit sensus"). *Ibid.*, 553.

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prefaced his own anti-Jewish polemic. If, as Richard W. Southern has argued, this other Peter embodied much of the distinctive intellectual spirit of the twelfth century, we can appreciate his words as encapsulating the still-prevalent approach to the Jews and their books. "Even today the Jews are to be allowed to live, because they are our enslaved book-bearers (*capsarii*), as they carry around the prophets and the law of Moses for the assertion of our faith. Not only in their books but also in their faces do we read of the passion of Christ."[18]

A NEW INTEREST IN HEBREW and rabbinic exegesis among Christian scholars, coupled with the need to retain the conviction that Judaism was essentially a fossil of biblical antiquity, raised serious questions and caused concern among various churchmen of the High Middle Ages. Did the use of Jewish sources as *auctoritates* conform to the underlying premises of Christian anti-Jewish polemic? Could granting a particular measure of truth to Jewish texts alter the Christian perception of contemporary Judaism? If rabbinic interpretation of the Old Testament was assumed to convey its literal meaning, positive Christian valuation of the *Hebraica veritas* and its historical sense could cast a presumption of significance and legitimacy on Jewish traditions, and this might in turn undermine the basic contrast between carnal Judaism and spiritual Christianity at the heart of the *Adversus Judaeos* tradition. The opposition between the letter and the spirit of the law had always been the foundation of Christianity's claims to validity, and this opposition had facilitated the conception of Judaism as stationary and antique, which had prompted seeking Jewish help on the literal sense of the Old Testament in the first place. When a Christian exegete of the twelfth century expounded the historical meaning of a biblical passage, perhaps even at the neglect of its Christological meaning, or when he approached a rabbi for advice in interpreting such a text, he assuredly did not deem his action an admission of the rectitude of Judaism. Yet in attributing relevance and importance to the contemporary Jewish understanding of the letter of the law, his exegesis could be construed as subverting the very set of theological assumptions that had spawned it.

One can thus appreciate the fear of literalism of Richard of St. Victor, himself a Hebraist, when he read the commentaries of Andrew of St. Victor and his successors.

> I have come upon a certain treatise of Master Andrew, which he wrote in explanation of Isaiah . . . , in which I have found several things imprudently taken for granted and argued in an unorthodox manner [*minus catholice*]. For in many instances in his treatise, the opinion of the Jews is cited as if it were not merely the Jews' but his own—and true. On the passage, "Behold a virgin will conceive and bear a son," he raises the questions or objections of the Jews and does not resolve them; and it appears as though he has given them victory, because he has left them [that is, their objections] as if they were insoluble. In matters of this sort those more informed are scandalized and those less informed are defamed, inasmuch as

[18] Peter of Blois, *Contra perfidiam Judaeorum*, *PL* 207: 825. Also see Southern, "Peter of Blois: A Twelfth Century Humanist?" in his *Medieval Humanism*, 105–32.

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even today there are those among his students who explain the aforementioned prophecy not in terms of the blessed Mary, but rather concerning a certain prophetess.[19]

One ought not to conclude that Andrew of St. Victor either denied the belief in the Virgin Birth or even challenged its allegorical prefiguration in Isa. 7:14. Enthusiastically applying his Hebraic knowledge to his commentary on an Old Testament text, he simply allowed his penchant for literalism to run free of theological concern and constraint. Nevertheless, Richard of St. Victor did compose his *De Emmanuele* to redress Andrew's error, which from Richard's perspective revealed not only a preference for the historical over the figurative interpretation of a single prophetic verse but also the ostensive preference of the literalist exegetes, indignantly dubbed *judaizantes nostri*, for Judaic over Christian hermeneutic. In Richard's view, Andrew and his disciples had interpreted the Old Testament independently of the New, paying little heed to the dictates of Catholic doctrine and ecclesiology, thereby enhancing the stature of Judaism at Christianity's expense. Richard's sentiments accompanied the transmission of Andrew's commentaries into the thirteenth and fourteenth centuries and are echoed even today in the monumental *Exégèse médiévale* of Henri de Lubac.[20]

If the new literalism of twelfth-century commentators like Andrew might undermine hallowed Christian teachings, the frequently concomitant reliance on rabbinic interpretations in explaining a scriptural text posed a related theoretical problem. His innocence notwithstanding, a churchman who sought instruction from a contemporary rabbi might unintentionally accredit the traditions of postbiblical Judaism, tacitly acknowledging that they embodied a meaningful understanding of the Old Testament. Yet, properly understood, rabbinic traditions also demonstrated that Judaism had not remained "stationary in useless antiquity" ever since the day of the crucifixion, as Augustine had presumed, and it was Augustine's assumption that had provided primary theological justification for tolerating Europe's growing Jewish community, as well as for the church's claim to have inherited God's biblical covenant from a stagnant, moribund Judaism. Medieval Christians hitherto owed most of their minimal knowledge of rabbinic traditions to Jerome, who considered them (as Peter Alfonsi did) an extension of the Jews' Old Testament perspective on divine and prophetic revelation.[21] The enthusiasm with which some prominent Christian exegetes now sought rabbinic instruction awakened doubts among others. In his gloss on a canon of Gratian's *Decretum*, which asserted the authority of the Hebrew text of the Old Testament, Rufinus reworked a statement of Jerome to establish that contemporary Jewish

[19] Richard of St. Victor, *De Emmanuele*, *PL* 196: 601–02. On the understanding of Scripture in a "Catholic sense," see Pelikan, *Growth of Medieval Theology*, 36–39.

[20] See Beryl Smalley, "A Commentary of Isaias by Guerric of Saint-Quentin, O.P.," *Miscellanea Giovanni Mercati*, 2 vols., Studi e testi, vols. 121–22 (Vatican City, 1946), 2: 391–95, and "Ralph of Flaix on Leviticus," *Recherches de théologie ancienne et médiévale*, 35 (1968): 52–68; Nicholas of Lyra, *Postilla litteralis*, in *Biblia sacra, cum glossis, interlineari et ordinaria, Nicholai Lyrani Postilla, ac moralitatibus, Burgensis additionibus, et Thoringi replicis*, 6 vols. (Venice, 1588), 3: 148FGH, 4: 21FGH–22BC, 89FG, 336B, 381G; and de Lubac, *Exégèse médiévale*, pt. 2, vol. 1: 362–67.

[21] De Lubac, *Exégèse médiévale*, pt. 2, vol. 1: 140–41, 238–47; Funkenstein, "Ha-Temurot be-Vikkuaḥ ha-Dat," 138 n. 49; and Merchavia, *Ha-Talmud bi-R'i ha-Natsrut*.

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books were no longer trustworthy. Jerome himself had emphasized that the Hebrew text of the Old Testament contained important evidence of the truth of Christian doctrine not found in Greek or Latin translations, unless the Jews had later distorted various passages that Christian polemicists had used against them. Rufinus maintained that such corruption had taken place on a grand scale, that now, because of the deceit of Jews who lived after the birth of Christianity, the Latin biblical text was more trustworthy than the Greek, the Greek more than the Hebrew. His concern was soon echoed by a series of influential canon lawyers: Johannes Faventinus, Huguccio, the author of the *Glossa palatina*, and Joannes Teutonicus.[22]

As I have argued elsewhere, the view of rabbinic Judaism as having deviated from its biblical predecessor came to maturity in the mendicant anti-Jewish polemic of the thirteenth century.[23] Twelfth-century churchmen did not perceive the extent and substance of talmudic literature and thus generally did not realize the troublesome ramifications that a new literal exegesis posed. Some writers, like Peter the Venerable of Cluny and the exegete Herbert of Bosham, did attempt to confront rabbinic teaching directly, but they failed to recognize the inconsistencies and contradictions in their treatments of the Jews and their traditions. In their studies of Herbert's commentary on Jerome's Hebrew Psalter, Beryl Smalley and Raphael Loewe have illustrated the impressive scope of this English scholar's Hebraic and Judaic knowledge. Impelled to understand the literal sense of Scripture despite its admitted inferiority to the spiritual, Herbert acquired facility in Hebrew and even Aramaic. He consulted contemporary rabbis, perused Jewish biblical commentaries and medieval textbooks on Hebrew grammar, and appears actually to have read from the Talmud and works of classical rabbinic midrash, and his work displays a remarkable familiarity with the subtleties of rabbinic hermeneutics, as well as with the history of the Masoretic Text. He did not shy away from criticizing Jewish opinion or from expressing sympathy with Richard of St. Victors' rebuke of *judaizantes nostri*. Yet knowledge gleaned from Jewish books remains the primary *auctoritas*, the foundation for Herbert's understanding of the

[22] Jerome wrote in his preface to the Pentateuch: "Sicubi tibi in translatione videor errare, interroga Hebraeos, diversarum urbium magistros consule: quod illi habent de Christo, tui codices non habent. Aliud est si, contra se, postea, ab Apostolis usurpata testimonia probaverunt, et emendatiora sunt exemplaria latina quam graeca, graeca quam hebraea." A. Gasquet *et al.*, eds., *Biblia sacra iuxta latinam vulgatam versionem ad codicum fidem*, 16 vols. (Rome, 1926–81), 1: 68–69. Gratian had accordingly recorded: "Libris veterum ebrea volumina, novis greca auctoritatem impendunt"; *Decretum*, D. 9, c. 6. Yet Rufinus wrote: "In tempore primitivo, antequam ecclesia per omnes partes orbis propagaretur, incorrupta erant et integra volumina Hebreorum atque Grecorum, procedente vero tempore, cum admodum christianus populus cresceret et multorum hereses in ecclesia germinarent, tam ab ipsis Iudeis ecclesie invidentibus quam ab hereticis hebraica et greca exemplaria corrupta sunt, sed magis hebraica quam greca, magis greca quam latina": *Summa decretorum*, ed. Heinrich Singer (Paderborn, 1902), 23. Compare the citation and questionable interpretation of Rufinus's gloss in Grabois, "*Hebraica veritas*," 625–26. Also see the comments on *Decretum*, D. 9, c. 6, in Johannes Faventinus, *Summa Decreti*, Biblioteca Vaticana, Reg. lat. MS. 1061, f. 53ra; Huguccio, *Summa Decreti*, Admont Stiftsbibliothek, MS. 7, f. 11v; *Glossa palatina*, Biblioteca Vaticana, Reg. lat. MS. 977, f. 3vb; and Joannes Teutonicus, *Glossa ordinaria*. I am grateful to Steven Horwitz of the Institute of Medieval Canon Law at the University of California, Berkeley, for supplying me with photoprints of the relevant portions of the manuscripts cited.

[23] Cohen, *The Friars and the Jews*, and "The Jews as the Killers of Christ in the Latin Tradition, from Augustine to the Friars," *Traditio*, 39 (1983): 1–27.

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Old Testament. He accused Christian commentators of corrupting the Hebrew text to support their messianic arguments. When he did understand a Psalm Christologically, he rarely took the time to expound it doctrinally, and he still evinced greater interest in the rejected Jewish teaching. He seems to have been completely at ease with Christological interpretation only when he could support it with rabbinic evidence; Christian exposition of the literal sense, his method suggests, should ideally comport with Jewish exegesis.[24] It is of particular significance, then, that Herbert's commentary itself provided fuel for the argument to invalidate rabbinic interpretations of Scripture, for he also suggested that rabbinic traditions embodied a heretical departure from the true, antique religion of biblical Israel. On a number of occasions, Herbert noted the differences in the beliefs and practices of ancient and medieval Jews; at times he accused contemporary rabbis of deliberately obscuring their ancestral traditions. Commenting on Ps. 2, he wrote:

> Modern Jewish scholars state that things in the Psalm . . . were explained by their ancient masters as pertaining to the messiah. They themselves do furnish the truth in their senseless little explanations; but, piecing together statements of the Psalm on the spur of the moment, as if they in fact wrote them, they prefer that *Quare fremuerunt etc.* [that is, Ps. 2] be interpreted these days with regard to King David—so as not to assent to ecclesiastical opinion, and to condemn the men of the Church. It is indeed the error of a perverse, depraved generation and its obstinate hatred of the truth, which prefers to deviate from its own masters and the authority of its sages, knowingly and intentionally to distort the Scriptures, so as not to espouse their ecclesiastical interpretation which its own ancient masters actually held![25]

Herbert neither recognized nor followed the implications of his logic. To the end of his life, he remained ambivalent toward the Judaic understanding of Scripture—maintaining his valuation of the literal sense and, accordingly, obliged to justify the persistence of his Christian faith.

Even though he lacked Herbert's Hebraic knowledge, Peter the Venerable, in his lengthy polemic against the Jews, also accused the postbiblical rabbis of abandoning the divine precepts of Scripture for the sake of the illegitimate teachings of men.[26] In particular, Peter criticized a number of talmudic homilies, whose logical absurdities and anthropomorphisms, he maintained, revealed the bestial, satanic character of contemporary Judaism. He voiced the rationalist spirit of his age, but in a line of argument diametrically opposed to that of Anselm of

[24] Beryl Smalley, "A Commentary on the *Hebraica* by Herbert of Bosham," *Recherches de théologie ancienne et médiévale*, 18 (1951): 29–65; and Raphael Loewe, "Herbert of Bosham's Commentary on Jerome's Hebrew Psalter," *Biblica*, 34 (1953): 44–77, 159–92, 275–98.

[25] Smalley, "A Commentary on the *Hebraica*," 57. It should be noted, in all fairness, that Andrew of St. Victor had denied the validity of rabbinic exegesis on a number of occasions but never so thoughtfully or convincingly as Herbert does here—hence the criticism of Andrew for Judaizing. See Smalley, *Study of the Bible*, 157–59.

[26] Peter the Venerable, *Adversus Judeorum inveteratam duritiem*, CCCM 58. In addition to the studies of polemic cited above, see Saul Lieberman, *Sheqi'in* (A Few Words on Some Jewish Legends, Customs, and Literary Sources Found in Karaite and Christian Works) (2d edn., Jerusalem, 1970), 27–42; Manfred Kniewasser, "Die antijüdische Polemik des Petrus Alphonsi (getauft 1106) und des Abtes Petrus Venerabilis von Cluny (+ 1156)," *Kairos*, new ser., 22 (1980): 49–76; and the introduction of Yvonne Friedman, CCCM 58: vii–lxx.

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Canterbury. For Peter, since the Jews spurned the blatantly rational truth of Christianity, they must have been less than human, and their blasphemous Talmud was to blame.[27] Peter attributed little value to a knowledge of Hebrew; Greek and Latin translations of the Bible afforded Christians precisely the same entrée to the truth, while the teachings of Jewish tradition were grossly unacceptable.[28] He proclaimed to the Jews:

> I extend to you before the whole world, O beastly Jew, that book of yours—yes, your book, that Talmud of yours, that illustrious doctrine of yours, which is to be preferred to prophetic books and all authentic teachings. . . . The Jewish Talmud cannot be that book of which it is said, "in thy book they are all recorded" [Ps. 139:16]. . . . Who besides Satan can teach such absurd things, and who besides the Jew can listen to, if not believe, them—that the reading of the Talmud can prejudice the power of God, that the incredible recitation of an infernal book can impede the will and mandate of God? For is that book of yours, O Jew, holier than the five books of Moses, holier than the books of the prophets, better or more worthy? . . . You have fought for so long against divine books with diabolical ones, and you have striven to tinge and obscure heavenly doctrine with the smoke of the infernal pit.[29]

Some historians have concluded that Peter hereby repudiated the conception of the Jew and Judaism that gave rise to the literalist exegesis of the twelfth century, that for him the medieval Jew was no longer the Jew of the Bible and rabbinic religion was nothing less than a perversion of real Judaism.[30] Yet a careful reading of Peter's anti-Jewish pronouncements reveals inconsistency and ambivalence. Like those of Herbert of Bosham, his comments suggest that contemporary Judaism and the religion of the Old Testament were not the same, but, without direct access to the Talmud, he failed to perceive the implications of his polemic. The Jews accordingly remained the guardians of the biblical books and tradition,

[27] Peter the Venerable, *Adversus Judeorum inveteratam duritiem*, CCCM 58: 57–58: "Patet ergo quia non ab huiusmodi canibus, sed a Iudaeis longe hiis deterioribus Christus ad occisionem ducendus a propheta praedictus est. Et quoniam fere idem est vel aduersus nugacissimas fabulas et primo auditu contemptibiles se quemquam disputando effundere, vel inanem aerem crebris ac validis ictibus feriendo vires lassare, sufficiat quod dictum est. Ad illud redeat sermo, propter quod de Iudaicis fabulis, quibus plus cunctis erroneis hominibus abundant, istud assumptum est. Est autem illud quod probare sermo intenderat, immo quod iam tam auctoritate, quam ratione probauerat, Christum temporalem regem intelligi non debere, Christi regnum terrenum, et finiendum accipi non oportere. Sufficere quidem possunt omni homini ad huius rei certitudinem quae praemissa sunt. Sed quia cum Iudaeo, qui nescio utrum homo sit, michi sermo est, adhuc aliqua addenda sunt. Nescio plane utrum Iudeus homo sit, qui nec rationi humanae caedit, nec auctoritatibus diuinis et propriis adquiescit."

[28] *Ibid.*, 99, 144–45.

[29] *Ibid.*, 125–26, 128, 139, 186. In a polemic against Islam, Peter even blamed the infidelity of Muhammad on the Jews and their rabbinic fables: Peter the Venerable, *Summa totius haeresis Saracenorum*, in James Kritzeck, *Peter the Venerable and Islam*, Princeton Oriental Studies, vol. 23 (Princeton, N.J., 1964), 206, and, in the English translation, 131–32: "And in order that the whole fullness of iniquity should come together in Mohammed, and that nothing should be lacking for his damnation and that of others, Jews were joined to the heretic. And lest [Mohammed] become a true Christian, the Jews, craftily providing for this man eager for novelties, whispered to Mohammed not the truth of the scriptures, but their own fables in which they abound even now. Thus Mohammed, instructed by the best Jewish and heretical doctors, produced his Koran and wove together, in that barbarous fashion of his, a diabolical scripture, put together both from the Jewish fables and the trifling songs of heretics."

[30] Amos Funkenstein forcefully maintained that Peter, breaking with the precedent set by Jerome, ceased to consider rabbinic Judaism a mere corollary of a literal understanding of Scripture. Peter, argued Funkenstein, instead considered "the Talmud the Jewish equivalent of the New Testament—that is, a different *nova lex*—and that, all would agree, was heresy"; "Ha-Temurot be-Vikkuaḥ ha-Dat," 140.

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distinguished for the unity and steadfastness of their loyalty to Mosaic law.[31] Their Talmud may have manifested a "bovine intellect," but Peter persisted in the traditional estimation of the Jews as blind to the truth of Scripture rather than as cognizant and deliberately unfaithful.[32] The Talmud, in other words, perpetuated the pre-Christian religion of ancient Israel, carnal understanding of the Bible and all. Rabbinic Judaism, albeit abominable, did not depart from the letter of the obsolete Mosaic covenant; it was not a heresy. In Peter's view, the "blasphemous, sacrilegious, ridiculous, and false" doctrine of talmudic Judaism emerged when Israelite prophecy ended; it antedated Jesus and thus comprised at least part of the religious outlook that Christianity came to replace.[33] Peter, therefore, could identify the Talmud's homilies with "the endless fables, and the traditions utterly foreign to the law of God, of which the Lord said to the Pharisees in the Gospel [Matt. 15:7], 'You have abandoned the precepts of God for the sake of your own traditions.'" Just as Peter Alfonsi had ignored the figurative, imaginative character of rabbinic Aggadah, so, too, Peter the Venerable attributed the error in talmudic interpretation to a mentality that insisted on interpreting a sacred text only literally, admitting "neither metaphor, nor allegory, nor any of the customary modes of figurative speech, through which all these things may rightfully be interpreted as applying to God."[34] European Jews still played their instructive part in the divine economy of salvation. "There is fulfilled in you, O wretched people, what your apostle by birth, ours in terms of doctrine, said of such things: 'The time will come when they will not stand wholesome teaching, but follow their own fancy and gather a crowd of teachers to tickle their ears. They will stop their ears to the truth and turn to mythology' [2 Tim. 4:3–4]."[35] Peter's anti-Jewish writings display no desire to promote proselytism; he consistently reaffirmed that the Jews ought not to be killed. Bearing the mark of Cain, the Jews should surely be subjugated and oppressed, but their traditions had not yet been judged incompatible with their toleration by Christian society.[36]

THIRTEENTH-CENTURY SCHOLASTICISM BROUGHT the academic discipline of theology to maturity. The innovative methodologies of previous generations, who were propelled by an unquenchable thirst for discovery and investigation, successfully

[31] Peter the Venerable, *Liber contra sectam sive haeresim Saracenorum*, in Kritzeck, *Peter the Venerable*, 252–53. Granted, the immediate purpose of this passage, in which Peter stated that Jews everywhere maintained copies of the same biblical books, is to counter Muslim assertions of the ancient loss and subsequent corruption of Scripture, but the implication that the Jews remained faithful to a canon of authentic, antique texts is unavoidable.

[32] Peter the Venerable, *Adversus Judeorum inveteratam duritiem*, CCCM 58: 1, 32, 42–43, 70, 89.

[33] *Ibid.*, 186.

[34] *Ibid.*, 152. Ludolf of Saxony echoed Peter's understanding of Matt. 15:7. See Ludolf of Saxony, *Vita Jesu Christi*, 1.88, 4 vols. (Paris, 1878), 2: 314–19.

[35] Peter the Venerable, *Adversus Judeorum inveteratam duritiem*, CCCM 58: 159.

[36] See *ibid.*, 141–42; and Peter the Venerable, *Letters*, ed. Giles Constable, 2 vols. (Cambridge, Mass., 1967), 1: 327–30. The lack of an urgent call for proselytism may in fact help explain the abusive tone of Peter's anti-Jewish pronouncements, in comparison to the softer tenor of his anti-Muslim polemic, whose purpose was perhaps conversion. See Kritzeck, *Peter the Venerable*, 21–23, and chaps. 3–5 *passim*; and Kritzeck, "Peter the Venerable and the Toledan Connection," in Giles Constable and James Kritzeck, eds., *Petrus Venerabilis, 1156–1956: Studies and Texts Commemorating the Eighteenth Centenary of His Death*, Studia anselmiana, vol. 40 (Rome, 1956), 176–201.

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transformed theology into a science, emancipated from the curricular and methodological prejudices of the church fathers. Schoolmen of the thirteenth century capitalized on the flurry of translations from Arabic and Greek into Latin, attempting to assemble all available knowledge in their exposition of sacred doctrine, just as they banded together in universities to formulate the intellectual syntheses that marked the age. No critical faculty was left uncultivated in what Jaroslav Pelikan has dubbed "the reintegration of the Catholic tradition." And the *summa*, the hallmark of thirteenth-century scholarship, was not complete without a reconciliation of all conflicting arguments, from authority or from reason, or without an intellectually compelling fusion of seemingly discordant ideas and texts. Significantly, the Scholastic synthesis did not rest in the ivory tower; it attempted to create a harmony between theory and practice, to apply the logic and conclusions of theological scholarship to social structures and individual lives. Scholasticism accordingly contributed to the flowering of an applied or missionary theology, intent on converting the infidel, rehabilitating the heretic, and insuring that all components of an organismically conceived Christian society worked toward the interests of the whole in every avenue of life. Schoolmen not only recovered, restored, and scrutinized ancient biblical and patristic texts but also developed new reference tools and methods of indexing these materials, which facilitated their availability to and utility for preachers as well as academicians. If the theological masters collected and studied works of Greek and Muslim philosophers, they also strove to eliminate all challenges to the Christian world view posed by these works. They implemented ecclesiastical directives to restrict the dissemination of dangerous ideas—above all those of Aristotle—and they condemned their colleagues who pursued the logic of Averroism beyond the limits of Christian responsibility. The *summa*, the universities, and the friars of the new mendicant orders who sought to realize the totalistic vision of their age—whether as academicians, preachers, inquisitors, or missionaries—all gave expression to the same ideological premise: intellectual activities were appropriately governed by Christian principles and directed toward Christian ends.[37]

These developments precipitated noteworthy changes in Christian attitudes toward the Jews and their books. Freed in some measure from its traditional enslavement to the exposition of Christian doctrine, thirteenth-century biblical exegesis refined its theories of signification in literary texts, formalizing the distinction between literal and spiritual senses and placing greater emphasis still on the former. Recognition of the value of the *Hebraica veritas* became more

[37] See Jean Leclercq, "The Renewal of Theology," in Benson and Constable, *Renaissance and Renewal*, 68–87; Nikolaus Haring, "Commentary and Hermeneutics," in *ibid.*, 173–200; Richard H. Rouse and Mary A. Rouse, "*Statim invenire*: Schools, Preachers, and New Attitudes to the Page," in *ibid.*, 201–25; Marie-Thérèse d'Alverny, "Translations and Translators," in *ibid.*, 421–62; Pelikan, *Growth of Medieval Theology*, 270–84; R. W. Hunt, "Manuscripts Containing the Indexing Symbols of Robert Grosseteste," *Bodleian Library Record*, 4 (1953): 241–55; Charles M. Radding, "Evolution of Medieval Mentalities: A Cognitive-Structural Approach," *AHR*, 83 (1978): 577–97; and Caroline Walker Bynum, *Jesus as Mother: Studies in the Spirituality of the High Middle Ages* (Berkeley and Los Angeles, 1982), 13, 82–109. On the Scholastic attempt to displace marginal and deviant elements in Christian society, see John Boswell, *Christianity, Social Tolerance, and Homosexuality: Gay People in Western Europe from the Beginning of the Christian Era to the Fourteenth Century* (Chicago, 1980), pt. 4.

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widespread; the number of Christians adept in Hebrew increased markedly; and schools of clerics labored to correct the text of the Vulgate, a process in which their acquaintance with the Hebrew Masoretic Text often proved significant.[38] The new Aristotelian rationalism also infused vitality into the endeavor to understand Old Testament teaching as purposeful in its original context. At the same time, however, new polemics appeared that rejected the contemporary Jew as no longer valuable in illuminating or enhancing the encounter between Latin Christendom and the Old Testament. The church's scholars gained considerable familiarity with postbiblical rabbinic tradition, and they now could perceive the historical evolution that separated antique biblical Judaism from the religion of medieval Jewry. Some deemed the European Jewish community unfit for toleration on theoretical grounds, given its abandonment of the stagnant, Old Testament way of life with which Augustine had justified its preservation. No longer was Judaism a mere foil that facilitated the Christological assertions of the polemicist. Anti-Jewish polemic assumed a much more direct and aggressive character; it aimed to undermine the presence and security of the Jew in a properly ordered Christian society. In all of this, one can discern an awakening to ideological inconsistencies that appeared to underlie the twelfth-century valuation of rabbinic instruction in Christian exegesis, and an effort to resolve them. Christian scholars finally grew sensitive to the interplay of their use of Jewish tradition with the logical demands of their *Adversus Judaeos* polemic.

The most telling evidence of these developments remains the well-known assault of the thirteenth-century church on the Talmud. Indicted before Pope Gregory IX by an embittered Jewish apostate, Nicholas Donin, in 1236, the Talmud was denounced in a series of papal bulls in 1239 and 1244, burned in Paris under papal direction in 1242, subsequently condemned by a commission of prelates and university masters in 1248, and attacked repeatedly by the church during the later Middle Ages.[39] The ecclesiastical treatment of the Talmud exemplifies the predilection of church scholars in the high Scholastic period to discover, to scrutinize, to understand, to classify, and actively to employ the contents of all available texts in their syntheses. Donin may have served as an important catalyst in the emergence of the church's new stance toward rabbinic Judaism, but the extent and impact of the new attitude quickly transcended the limited sphere of his personal influence. Two hundred years earlier, even someone like Donin could not have elicited the forceful response enunciated by the papacy in the 1230s and 1240s, and, even without Donin, the ideological climate of thirteenth-century

[38] Among others, see Smalley, *Study of the Bible*, chap. 6; Raphael Loewe, "The Medieval History of the Latin Vulgate," in P. R. Ackroyd *et al.*, eds., *The Cambridge History of the Bible*, 3 vols., (Cambridge, 1963–70), 2: 148–52; Samuel Abraham Hirsch, *Early English Hebraists: Roger Bacon and His Predecessors* (London, 1905); and Altaner, "Zur Kenntnis."

[39] Yitzhak Baer, "Le-Viqqoret ha-Vikkuḥim shel R. Yeḥi'el mi-Paris ve-shel R. Moshe ben Naḥman" (The Disputations of R. Yechiel of Paris and of Nachmanides), *Tarbiz*, 2 (1931): 172–87; Judah M. Rosenthal, "The Talmud on Trial," *Jewish Quarterly Review*, new ser., 47 (1956): 58–76, 145–69; Solomon Grayzel, "The Talmud and the Medieval Papacy," in Walter Jacob *et al.*, eds., *Essays in Honor of Solomon B. Freehof* (Pittsburgh, Pa., 1964), 220–45; Merchavia, *Ha-Talmud bi-R'i ha-Natsrut*, 227–420; and Cohen, *The Friars and the Jews*, chaps. 6–7.

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Christendom was such that a careful investigation of contemporary Jewish belief and practice was only a matter of time. It is telling that, in the realm of metaphysics, thirteenth-century Christian philosophers expended great energy to grapple with the systems of Arabic and Jewish thinkers, so like and yet unlike their own, feeling compelled to assimilate them and to overcome them simultaneously. The Jew Moses Maimonides thus commanded respect as a philosophical authority in the Christian academy; his philosophical formulations served as a model for blending Aristotelian science with faith in the God of the Bible and for demonstrating the limits of rational inquiry. William of Auvergne and Thomas Aquinas frequently appropriated the rationalist Maimonidean exegesis of the Mosaic commandments, asserting their rightfulness and purpose in their original setting and thereby promoting the Christian study of the Old Testament in its literal sense.[40] Nevertheless, this positive estimation of Maimonidean thought did not prevent Dominican inquisitors from burning the works of Maimonides as heretical, Thomas Aquinas from reproaching him for abandoning the literal meaning of Scripture, and Giles of Rome from condemning him for forsaking the rational truth of biblical revelation.[41] Given such an intellectual climate, the amplitude and theological-historical implications of talmudic literature provoked a swift, deliberate reaction.

The Christian encounter with the Talmud demonstrates that growing numbers of influential churchmen were acquiring an interest in rabbinic texts and recognizing the importance of greater familiarity with them. On the one hand, a rabbinic interpretation of Scripture continued to inform Christian understanding of the literal, historical sense of the Old Testament. On the other hand, in its discovery of the Talmud, the thirteenth-century church also discerned the postbiblical character of rabbinic tradition and perceived the threat that it posed to the Augustinian notion of what the Jews and their books were supposed to be. Talmudic and midrashic texts thus had to be mastered and persecuted at the same

[40] Beryl Smalley, "William of Auvergne, John of LaRochelle and St. Thomas of Aquinas on the Old Law," in *St. Thomas Aquinas, 1274–1974, Commemorative Studies*, 2 vols. (Toronto, 1974), 1: 10–71; Jacob Guttmann, "Der Einfluss der maimonidischen Philosophie auf des christliche Abendland," in W. Bacher *et al.*, eds., *Moses ben Maimon: Sein Leben, seine Werke und sein Einfluss*, 2 vols. (1908; reprint edn., Hildesheim, 1971), 1: 135–230; and Wolfgang Kluxen, "Maimonides im lateinischen Abendland als Beispiel einer christlich-jüdisch Begegnung," in Paul Wilpert, ed., *Judentum im Mittelalter: Beiträge zum christlich-jüdischen Gespräch*, Miscellanea mediaevalia, vol. 4 (Berlin, 1966), 146–66. On Aquinas's resulting theological evaluation of Mosaic law, see, among others, Yves M.-J. Congar, "Le sens de l'économie salutaire dans la 'théologie' de S. Thomas d'Aquin (*Somme théologique*)," in Erwin Iserloh and Peter Manns, eds., *Festgabe Joseph Lortz*, 2 vols. (Baden-Baden, 1958), 2: 73–122; M.-D. Chenu, "La théologie de la loi ancienne selon Saint Thomas," *Revue thomiste*, 61 (1961): 485–97; Amos Funkenstein, "Gesetz und Geschichte: Zur historisierenden Hermaneutik bei Moses Maimonides und Thomas von Aquin," *Viator*, 1 (1970): 147–78; and Shlomo Pines, "Saint Thomas et la pensée juive médiévale: Quelques notations," in G. Verbeke and D. Verhelst, eds., *Aquinas and the Problems of His Time*, Mediaevalia lovanensia, ser. 1, vol. 5 (Louvain, 1976), 118–29.

[41] On the inquisitorial burning of Maimonides' works, see Cohen, *The Friars and the Jews*, 52–60 and nn. 1–15. Among the works that Cohen cited, see, especially, the intriguing arguments of J. L. Teicher; Teicher, "Christian Theology and the Jewish Opposition to Maimonides," *Journal of Theological Studies*, old ser., 43 (1942): 68–76. For Aquinas's rebuke of Maimonides, see Aquinas, *In IV Sententiarum*, 48.2.3, ad 6; and Hans Liebeschütz, "Eine Polemik des Thomas von Aquino gegen Maimonides," *Monatsschrift für die Geschichte und Wissenschaft des Judentums*, 80 (1936): 93–96. Also see Giles of Rome, *Errores philosophorum*, 12.1–11, ed. Josef Koch and trans. John O. Riedl (Milwaukee, Wis., 1944), 58–67.

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time; whatever utility they proffered had to be liberated from the biblically inauthentic—and therefore ultimately intolerable—contemporary Jewish community that preserved and defended them. William of Auvergne, Albert the Great, and Hugh of St. Cher openly applied their knowledge of rabbinic sources in their writings, and yet they all participated actively in the Parisian condemnations of the Talmud. Raymond de Peñaforte—who served as papal confessor when Gregory IX received Donin at the curia, who stood at the helm of the Order of Friars Preachers when the Dominicans played a leading role in the *auto-da-fé* of 1242 against the Talmud, and who clearly shared the view of contemporary postbiblical Judaism as a heretical departure from the biblical norm—vehemently advocated that clerics study Hebrew and use rabbinic sources in preaching to the Jews. Raymond opened a number of schools for training mendicant friars in Hebrew, and Pablo Christiani and Raymond Martini, his two best-known disciples, both based their polemics against the Jews on the premise that rabbinic texts bespoke the truth of Christianity.[42] Most significantly, as the church grew more conscious of the complex linkage between its attitude toward Judaic learning and its treatment of the Jews, it began to voice concern over this troublesome relationship. The *Summa theologiae* attributed to Alexander of Hales weighed the evils of the Talmud against the positive testimonial function served by the Jews' presence in Christendom. "Since they must observe the doctrine of that book as law," the treatise concludes, "their books, in which blasphemies are contained, are to be burned"—although the Jews and their legitimate Old Testament rites did warrant toleration.[43] Later in the 1240s, when the Jews protested to the papacy over the ecclesiastical attack on the Talmud, the key issue set the alleged postbiblical—and therefore heretical—nature of rabbinic literature against the Christian duty to endure the survival of biblical Judaism. Popes Gregory IX and Innocent IV had deemed the Talmud a distortion of Judaism and had consigned it to the flames. As a result, Innocent later recounted to Louis IX of France,

> the Jewish masters of your kingdom recently asserted before us and our brothers, that without that book which in Hebrew is called "Talmut," they cannot understand the Bible and their other statutes and laws in accordance with their faith; we then, bound as we are by the divine command to tolerate them in their Law, thought fit to have the answer given them that we do not wish to deprive them of their books if as a result we should be depriving them of their Law."

Innocent added that he had designated the chancellor of the University of Paris, Eudes of Chateauroux, to inspect rabbinic books, and "of these he should tolerate such as he will find may be tolerated, in accordance with the [aforementioned]

[42] Cohen, *The Friars and the Jews*, 62, n. 20, 103–69, and "The Jews as the Killers of Christ," 26.

[43] Alexander of Hales, *Summa theologiae*, 2–2.161.1, 5 vols. (Quaracchi, 1924–48), 3: 729. For a translation, see Robert Chazan, ed., *Church, State, and Jew in the Middle Ages* (New York, 1980), 44–46. Also see Jacob Guttmann, "Alexandre de Hales et le judaïsme," *Revue des études juives*, 19 (1889): 224–34; W. Lampen, "Alexander von Hales und der Antisemitismus," *Franziskanische Studien*, 16 (1929): 1–14; Venicio Marcolino, *Das Alte Testament in der Heilsgeschichte: Untersuchung zum dogmatischen Verständnis des Alten Testaments als heilsgeschichtliche Periode nach Alexander von Hales* (Münster, 1970); and Smalley, "William of Auvergne," 25.

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divine command."[44] The legatine commission convened at the University of Paris and condemned the Talmud for its "innumerable errors, abuses, blasphemies, and wickedness." The language of this formal pronouncement was terse, but Eudes argued more discursively in an earlier letter responding to the papal demand for his investigation.

> The said books were full of errors, and . . . the veil covers the heart of these people to such a degree, that these books turn the Jews away *not only from an understanding of the spirit, but even of the letter*, and incline them to fables and lies. From this it is clear that the Jewish teachers of the Kingdom of France uttered a falsehood to Your Holiness and to the sacred fathers, the Lords Cardinals, when they said that without these books, which in Hebrew are called "Talmud," they cannot understand the Bible and the other precepts of their laws, in accordance with their faith.[45]

Despite recent scholarly protestations to the contrary, the charge of talmudic heresy—that the books under indictment obviated Jewish fidelity even to the letter of the Old Testament—remained at the heart of ecclesiastical concern, both during the thirteenth century and beyond.[46] One must also note that Eudes did not rest with his summary condemnation of the Talmud and his insistence that it not be restored to the Jewish community. As chancellor of the University of Paris, he also supervised a massive effort by clerics to study the Talmud and present those of its contents with a potential interest to a Christian reader in a manner befitting his needs. The result was a written compilation over two hundred folio pages long, usually entitled *Extractiones de Talmut*, which first amassed in the order of their appearance some two thousand talmudic texts deemed noteworthy, then grouped them under thirteen subject classifications (for example, "Concerning the authority of the law which is called 'Talmud,'" "Concerning blasphemies against Christ and the Blessed Virgin," and so forth), and finally appended some shorter lists,

[44] Solomon Grayzel, ed., *The Church and the Jews in the XIIIth Century* (rev. edn., New York, 1966), 275.

[45] *Ibid.*, 278–79 n. [Emphasis added]. For a list of the masters who signed the report of the legatine commission, see Heinrich Denifle, *Chartularium universitatis parisiensis*, 4 vols. (Paris, 1889–97), 1: 210–11.

[46] See Joel E. Rembaum, "The Talmud and the Popes: Reflections on the Talmud Trials of the 1240s," *Viator*, 13 (1982): 203–23. Arguing that Innocent IV in 1247 dropped the charge of heresy from the papal opposition to the Talmud, Rembaum dismissed the significance of Innocent's own decretalist commentary—which rationalized the papal condemnations of the Talmud on the basis of its deviation from the Mosaic law—with the unconvincing suggestion that "it is possible that he formulated this statement on the basis of Gregory's bulls of 1239 and his own letter of 1244 prior to his being approached by the Jews in 1247." *Ibid.*, 216 n. 81. Innocent wrote his commentary on the *Decretales* between 1246 and 1254. In the case of such an accomplished scholar, one may not assume that the final version of his work would have undercut the theoretical basis for his own actions only several years earlier. See Innocent IV, *Commentaria . . . super libros quinque decretalium*, ad X.3.34.8, 2 vols. [Frankfurt, 1570], 1: 430; and Ch. Lefebvre, "Sinibale dei Fieschi (Innocent IV)," in *Dictionnaire de droit canonique*, 7 vols. (Paris, 1935–65), 7: 1031. Rembaum failed to come to grips with the arguments of Benjamin Z. Kedar. See Kedar, "Canon Law and the Burning of the Talmud," *Bulletin of Medieval Canon Law*, 9 (1979): 78–83. And Rembaum overlooked the fact that the rationale of Innocent's commentary for the burning of the Talmud was echoed in the subsequent commentaries on the same decretal by Hostiensis, Joannes Andreae, and Franciscus Zabarella, by the fourteenth-century inquisitor Nicholas Eymeric, and by commentators at the court of the fifteenth-century antipope Benedict XIII. See Eymeric, *Directorium inquisitorum*, 2.46.3–4 (Rome, 1578), 244–45; and A. Domingues de Sousa Costa, "Canonistarum doctrina de Judaeis et Saracenis tempore Concilii constantiensis, *Antonianum*, 40 (1965): 55–70.

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including a collection of texts about the mechanisms of the rabbinic legal system.[47] Eudes' undertaking manifests the Scholastic inclination for using texts to serve the higher purposes of Christendom. Discoveries in the classroom demanded packaging that would facilitate their application in the world at large—in the case of the Talmud, to the pastoral interpretation of Scripture, the mendicant mission to the Jews, and the justification for persecuting Jewish books. As Richard Rouse and Mary Rouse have concluded, "Tools [which afforded easier access to important texts], mendicants, and (to a large degree) university are all responses to the same demand, the Church's need for a clergy properly trained and provided with the necessary books to preach and to minister to a Christian society."[48]

A sharp distinction between the utilitarian employment of Jewish exegesis and the tacit countenance of an inherently illegitimate rabbinic tradition also pervades the biblical commentaries of the Parisian master Nicholas of Lyra, whose academic career spanned the late thirteenth and early fourteenth centuries. Nicholas accepted the Thomistic definition of the literal sense of the Old Testament as that which its writer consciously intended, but he argued that such an intention could indeed have been Christological. The primary meaning of the same biblical text could therefore include both historical and Christological dimensions.[49] Throughout his postils, Nicholas drew extensively on the glosses of Rashi (Solomon ben Isaac of Troyes, 1040–1105) and on other rabbinic works. Yet, avoiding the logically problematic conclusions of earlier Hebraists, Nicholas did not entangle his knowledge of Hebrew in the a priori legitimization (whether expressed, implicit, or even unintentional) of the instruction of European rabbis. His Hebraica did not depend on the Jews.[50] He advocated and pursued Hebraic study

[47] The earliest extant copy of the *Extractiones* is in the Bibliothèque Nationale, Paris, lat. MS. 16558. For a detailed description of the manuscript, see Merchavia, *Ha-Talmud bi-R'i ha-Natsrut*, chap. 13.

[48] Richard H. Rouse and Mary A. Rouse, *Preachers, Florilegia, and Sermons: Studies on the Manipulus Florum of Thomas of Ireland* (Toronto, 1979), 6–7.

[49] See, for example, Nicholas's discussion of the biblical instructions for preparing the original paschal sacrifice; *Postilla litteralis*, on Exodus 12.1, 13.10, in *Biblia sacra, cum glossis*, 1: 145F–146B, 150GH: "One must understand the burning of the paschal lamb, as well as other sacrifices, in two respects. One concerns the [historical] moment, namely, the situation of the people departing from Egypt. The second is the signification (*figuratio*) of the lamb, designating the passion of Christ, which was prefigured in them [that is, the sacrifices]. The Apostle [Paul] accordingly states, 'Everything happened to them as a sign (*in figura*)' [1 Cor. 10:11]; and Rabbi Solomon states—and likewise the ancient Hebrew doctors—that all the prophets prophesied exclusively concerning the days of the Messiah. Therefore, since Moses was the greatest prophet, everything which he wrote is ordained for Christ; and as a consequence, there is a twofold meaning [duplex est sensus] in [the instructions for] offering the paschal lamb: One is the situation of the people departing from Egypt; this is literal and first [in time], and this I intend to explicate first. The other is the prefiguration of Christ who would someday suffer, and on this meaning I shall touch briefly at the end. For although this is last in chronological order, it is nevertheless primary in intention—like an objective, in relation to those things which have been ordained toward an objective [sicut finis respectu eorum, quae sunt ad finem ordinata]." The most extensive studies of Nicholas and his reliance on Hebraic sources remain those of Herman Hailperin. See Hailperin, *Rashi and the Christian Scholars*, esp. 184–91, "Nicholas de Lyra and Rashi: The Minor Prophets," in *Rashi Anniversary Volume*, American Academy for Jewish Research Texts and Studies, vol. 1 (New York, 1941), 115–47, and "The Hebrew Heritage of Medieval Christian Biblical Scholarship," *Historia Judaica*, 5 (1943): 133–54. On the text adduced in this note, see Obermann, *Forerunners of the Reformation*, 286.

[50] Compare the conclusion of Herman Hailperin; Hailperin, "Yaḥaseihem shel Ḥakhmei-ha-Notsrim 'el Ḥakhmei Yisra'el, ve-Hashpa'ato shel RaSh"I 'al Mefarshei ha-TaNa"Kh ha-Notsriyim" (Intellectual Relations between Christian and Jew with Special Reference to Rashi and Nicolas de Lyra),

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in the service of exegesis even in the absence of the Jews, who were expelled from France in 1306, and he maintained that talmudic Judaism and its contemporary spokesmen had deviated deliberately from the faith of their antique biblical forebears and were essentially heretical as a result. One could no longer casually presume Jewish exegesis to be authoritative. In the prologue to his monumental *Postilla litteralis*, Nicholas decried the obfuscation of the literal understanding of Scripture in his day, which he attributed primarily to the errors of scribes and translators. "In order to attain the literal truth in the Scripture of the Old Testament, one must resort to the books of the Hebrews." In the same passage, however, Nicholas echoed the warning of Rufinus. "In this one must still be cautious indeed with regard to those passages in Old Testament Scripture which speak of the divinity of Christ and its consequences. The Jews have corrupted some of them in defense of their error." To his explanation for adducing the literalist commentaries of Rashi, "who among the Hebrew doctors has spoken most reasonably," Nicholas juxtaposed a reminder to his readers that absurd Judaic teachings ought not to be condoned.[51] Rashi himself elicited rebuke from Nicholas for concealing the Christological truth of the Bible, and Nicholas typically substantiated his charges with references to other texts of presumably ancient Jewish interpretation that the medieval rabbi had deliberately abandoned. For example, to "the error of the Hebrews, who say that in these three chapters [of Isaiah], namely 7, 8, and 9, mention is not made of Christ," Nicholas responded, "In order to disallow this falsehood and to declare the truth, one must depend above all on the Aramaic translation [of the Old Testament] which among the Hebrews is called 'Targum' and is of such great authority among them that no one has dared to contradict it." Nicholas similarly cited the Septuagint to disclose Rashi's corruption of Isa. 9:5 and the glosses of Rabbi Moses ha-Darshan of Narbonne to refute Rashi's exegesis of Ps. 79. All of these sources, Nicholas believed, antedated the talmudic heresy.[52] Rashi's "perversity" allegedly led him to self-contradiction in the understanding of Isa. 55:5. And, in his discussion of Ps. 2, Nicholas echoed the polemic of Herbert of Bosham; he indicted Rashi for

Proceedings of the Rabbinical Assembly of America, 7 (1940): 223. Hailperin wrote that, even if "all the Jews in the world of that day were to have converted to Christianity, Christian scholars [that is, Nicholas] would not have ceased to deal with Hebraic studies and Hebraic culture in the least."

[51] *Biblia sacra, cum glossis*, 1: 3GH.

[52] *Ibid.*, 3: 203FGH–204B, 4: 21FGH, 24GH–25BCD. The notion that a full acceptance of Targumic interpretations of Old Testament prophecies would lead directly to Christianity still appears in modern Christian scholarship. See, for instance, Roger le Déaut, *The Message of the New Testament and the Aramaic Bible (Targum)*, trans. Stephen F. Miletic, Subsidia biblica, vol. 5 (Rome, 1982), 43–55. "Frequently the Aramaic translations represent real theological progress (especially in so far as explicitation is concerned) when compared to the Hebrew text. As such, they form one stage towards the revelation of the New Testament." Déaut's disclaimer of sympathy with the medieval polemicists notwithstanding, he, too, suggested that rabbinic tradition deliberately suppressed Aramaic translations in early Jewish biblical interpretation and that these could have served to bring Jews closer to the church. "But the primary interest of the Aramaic versions is that they could be understood as a *praeparatio evangelica* ('preparing the way for the Gospel'). The best proof of their usefulness for understanding the content itself of the New Testament and Christian apologetics resides in the defiance and in the growing opposition of the Rabbis against this type of writing, leading to only one 'authorized' recension, which conformed to official teaching, namely the Onkelos translation of the Torah and the recension of Jonathan ben Uzziel for the Prophets." Déaut, *Message of the New Testament*, 43, 54, 51.

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openly conceding the messianic message of the text and for demanding at the same time that "for the sake of responding to the heretics [that is, Christians] it be expounded as concerning David."[53] Rashi epitomized for Nicholas the most learned of contemporary Jews (*Judei moderni*), whose Hebraic knowledge could serve the Christian exegete of the Old Testament but whose adherence to deviant talmudic Judaism merited outright condemnation and rejection. Nicholas accordingly clarified and reiterated the warning of Richard of St. Victor against overestimating the reliability of Jewish biblical interpretation. The *Postilla* denounces Andrew of St. Victor for joining Rashi in his *perversitas*,[54] chastises those Catholic doctors who, like Andrew, "Judaize" more than Rashi himself ("magis Iudaizant quantum ad hoc, quam ipse Rabbi Salomo"),[55] and even faults an interpretation of Thomas Aquinas for favoring the error of the Jews over the doctrine of the church.[56] As Hebraist, commentator, and theologian, Nicholas succeeded not only in appropriating literalist exegesis from medieval Jews but also in recognizing the ideological dangers in according their texts intrinsic value.

IT IS INSTRUCTIVE THAT IN THE PERIOD of the most intense Christian study of Judaica new polemics undermined the security of European Jewry. Whereas in the early medieval period ignorance of rabbinic teaching had perpetuated the nonaggressive *Adversus Judaeos* polemic of the church fathers, which was grounded in the notion of a Jewish religion that had remained stagnant and moribund since the first century, the growing interest in traditional Jewish learning ultimately led to disavowal of the old presupposition and its logical implications. A more direct and hostile type of polemic emerged, and alongside it a new Christian view of Jewish history, one that perceived a major qualitative transformation in the nature of Judaism and Jewish literature since the end of biblical times and the birth of Christianity. Such a schema underlay the novel idea of Thomas Aquinas and Nicholas of Lyra that first-century Jews actually recognized Jesus as Messiah and Son of God but spitefully killed him nonetheless. It facilitated the aggressive proselytizing of Raymond de Peñaforte and his confreres, as evidenced in the monumental *Pugio fidei* of Raymond Martini.[57] And, in terms of the new bifurcation between the literal truth of Scripture and the beliefs of contemporary Jews, this notion was expressed quite succinctly by William of Auvergne. In his *De legibus*, William first affirmed the primacy and authenticity of the literal—as opposed to the prefigurative—meaning of the Old Testament:

[53] *Biblia sacra, cum glossis* 3: 88DEFGH, 4: 89H–90B.

[54] On Isa. 53, see *ibid.*, 4: 89FG.

[55] On Ps. 44 (Ps. 45 in the Masoretic Text), see *ibid.*, 3: 148FGH. On Hos. 2:15 (Hos. 2:17 in the Masoretic Text), see *ibid.*, 4: 336B. On Mic. 5:2 (Mic. 5:1 in the Masoretic Text), see *ibid.*, 4: 381G.

[56] *Ibid.*, 4: 21FGH–22BC. Concerning the nonmessianic, Jewish interpretation of Isa. 8:3, Nicholas reported: "Hoc dictum videtur sequi Thomas de Aquino in quadam postilla, quae sibi attribuitur tanquam ex eius dictis reportata, ubi solvuntur argumenta que solent fieri ad contrarium. . . . Hoc dictum videtur nimis favere errori Hebraeorum. . . . Solutiones Thomae . . . non valent: quia falsum assumunt." For Aquinas's statement, see his *In Isaiam prophetam expositio*, 8.1, in Stanislaus Eduard Fretté and Paul Myaré, eds., *Opera omnia*, 34 vols. (Paris, 1874–80), 18: 722–25.

[57] Cohen, "The Jews as the Killers of Christ," 19–27.

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Why was it necessary for the law to express a spiritual meaning, at a time when we were not yet in existence? And after the Christian Church was established, it was truly not necessary that the law speak to us figuratively, since the truth of the Gospel itself had come to us clear and unconcealed. So by all means it is necessary that such things [Old Testament precepts] had a literal sense. And it is most improbable that the entire Jewish nation, the people of the law—in which everyone, from the most distinguished to the most obscure, was trained in the law and studied it—for so many thousands of years, would not have detected such an error [that is, that they understood the law incorrectly].

Yet contemporary Jews, continued William, no longer understood this literal truth.

If anyone should ask why the Jews themselves today do not accept what we have stated and written concerning the ostensive absurdities [of the truly rational Mosaic laws], we would respond that at the time of their enactment, [their reasons] were most apparent to the [Jewish] sages. Yet this knowledge vanished from among the Jews, because of the passage of time, their suffering, their dispersion, and above all their neglect of the study of the law. Such neglect was caused in part by the greed to which they are certainly prone, and in part by the love of Gentile philosophy.[58]

In the high medieval academy, increased study of a minority viewpoint led not to greater toleration but to greater bifurcation between the circumscribed value of such an outlook and its despised protagonists. Twelfth- and thirteenth-century scholars may well have refined numerous hermeneutic techniques in pondering the significative character of theological language. Yet, as G. R. Evans has noted in a recent monograph, modern standards of textual criticism eluded them, inasmuch as their reading of a text still had to fit the predetermined mold of their theology. Modes of interpretation may have changed, but not the fundamental doctrinal assumptions that antedated them.[59] For the medieval Jew, the results were dire. As Scholasticism facilitated an understanding of his books, it nearly eliminated his theologically grounded utility in European Christendom.

[58] William of Auvergne, *De legibus*, chap. 15, in *Opera omnia*, 2 vols. (Paris, 1674), 1: 46–47. Also see Jacob Guttmann, "Guillaume d'Auvergne et la litterature juive," *Revue des études juives*, 18 (1889): 243–55; and Smalley, "William of Auvergne," 27–46.

[59] Evans, *Language and Logic of the Bible*, 164–67.

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Comment:

DAVID BERGER AND JEREMY COHEN HAVE CONTRIBUTED valuably to the recently resumed discussion of an ancient debate. Thanks largely to the revulsion caused by the horrors of the "Final Solution" rather than to any prior development in Christianity, non-Jewish culture, or historiography, the attitudes of most non-Jews in the West toward Jews have undergone a radical change. Non-Jewish scholars have recognized that they need more, and more objective, knowledge of Jewish history, and, as a result, they have paid greater attention to the work of historians of the Jews. Much more striking, however, has been the effect of these new attitudes on the history of Christianity. In a remarkably prescient book published in 1934, James Parkes asserted that theological conceptions—or misconceptions—developed by Christians in the first three centuries of the Common Era laid the foundations on which modern anti-Semites had reared their racial and economic propaganda.[1] That now-famous book received little attention at the time, but the question of the responsibility of Christianity for what had happened to Jews was raised again after the Second World War—this time inescapably, by Parkes and many other Christians.

Critical reexamination of Christian treatment of Jews throughout history has become a major Christian enterprise. Christian scholars have attempted to recognize and, where acceptable, to eliminate Christian ideas detrimental to Jews. Recent efforts to reinterpret St. Paul's ideas about the salvation of Jews and the validity of Judaic law indicate how far this reexamination has advanced.[2] Indeed, in *Faith and Fratricide*, a work in which history and theology are inextricably combined, Rosemary Reuther, a Christian, went so far as to ask whether Christology, the fundamental historical Christian conception of Jesus, was not in some measure responsible for what later happened to Jews.[3] It is ironic. For centuries, Christian theology dictated how non-Jews should conceive of Jewish history, but now more objective knowledge about Jews and Jewish history is affecting Christian doctrine and theology and the history of Christianity.

To Jews, of course, it came as no news that Christians had fundamentally misunderstood Judaism and abused Jews horribly for centuries. Nor was the integrity of Christian theology their responsibility. But the changed climate did

[1] Parkes, *The Conflict of the Church and the Synagogue* (New York, 1969), 375–76.

[2] See Krister Stendahl, *Paul among Jews and Gentiles* (Philadelphia, 1976); Lloyd Gaston, "Paul and the Torah," in Alan Davies, ed., *Antisemitism and the Foundations of Christianity* (New York, 1979), 48–71; and John G. Gager, *The Origins of Anti-Semitism* (New York, 1985), 193–264.

[3] Reuther, *Faith and Fratricide* (New York, 1974). For the reactions to Reuther and her response to them, see Davies, *Antisemitism and the Foundations of Christianity*.

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mean that many historians of Jews and Judaism, whether believers or not, could turn their critical attention to Christianity in a way that had been dangerous to do openly before. In addition to informing Jews and non-Jews about Jewish history, and as part of that task, Jewish historians could now emphasize characteristics of Christian thought and action through the ages—and their consequences for Jews—that Christians, and indeed non-Jewish historians in general, had heretofore refused to recognize.

This critical reexamination of the way professed Christians treated Jews in the past has increased our understanding notably. Yet, despite marked differences in their perspectives, most historians who have dealt with the subject have accepted that the central issue in the conflict is religion, and many have also taken for granted that the religious beliefs involved are not themselves susceptible of objective analysis. From an empirical point of view, however, the basic question is why, for millenia, millions of human beings were locked in a conflict with results ultimately horrifying to both sides. And the explanation has to be made in terms far more objective than those provided by the rationalizations of the protagonists in the conflict, whether Christians or Nazis. Their rationalizations have to be examined as symptoms of the conflict rather than its explanation. Yet much of the recent scholarship on the problem has concentrated instead on the ideas of the protagonists and implicitly accepted that the fundamental explanation of the conflict is the ideas or beliefs prescribed by the authorities of Christianity and Judaism, primarily those of Christianity, and that the issue is one between Christians and Jews. There are obvious reasons for this.

A moral question about Christian responsibility initially inspired much of the interest in this kind of scholarship, and many who pursued it identified themselves with Christianity or Judaism. They tended to think of the people whose actions they were studying above all as Christians and Jews rather than simply as human beings, and the natural question for them to ask was where had Christian thinking gone wrong. Hence, even historians who are minimally ethnocentric have tended to categorize the people whose history they have examined primarily as Christians and Jews and to focus on some of their professed religious ideas as the primary cause of the conflict, but they have not used the historical method they would apply to secular ideologies to explain the ideas or beliefs of either religion.

The articles of Berger and Cohen conform to this latter approach. They are essays in the history of ideas rather than investigations of the causes of rationalizations. Their focus is restricted to what might be called the professional opinions and judgments of medieval religious scholars on specific disagreements, with particular attention to the impact of a new awareness of Judaic ideas on the thinking of twelfth-century Christian theologians. Knowledge of what else might have been in these people's minds, or how social and psychological pressures might have influenced their thoughts and emotions, or what other people were doing and thinking is assumed or disregarded. And, if the debates they describe are distinctively medieval, the central issue on which they concentrate is one on which Christians and Jews have consciously disagreed throughout the Common Era—and still do.

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CHRISTIANITY BEGAN IN A DISAGREEMENT among Jews about ultimate religious authority and the Torah of Israel. As Christianity and Judaism became increasingly distinct in the first century of the Common Era, the central issue became the Christian belief that Christ was the Messiah or divine and that there was a new covenant not restricted to Jews that freed believers from literal observance of the Law. Jews rejected this belief as a deviation that abandoned monotheism and blasphemed God by ascribing the imperfections of humanity to him and depicting him as changing his mind. Those objections were posed again in the sharpest terms in the Middle Ages in the polemics discussed by Berger, but in the Roman period the teachers who were developing the talmudic Judaism to which the immense majority of Jews remained faithful wasted little time on them. Since their Judaism concentrated on the Law (which Christians no longer practiced), did not consider Jesus and what was recounted of him as divine revelation, and, until the time of Maimonides, turned away from Greco-Roman thought and any philosophical problems it might pose, Christian ideas were simply irrelevant. In striking contrast, however, Christian thinkers could not disregard Judaism, for Christianity was indelibly marked at its birth by its traumatic emergence from Judaism and remained inextricably linked with biblical Judaism. Jews were the actors in Christianity's central event, and it was heavily Judaic in its morality and liturgy and in its myth of origins, which was enshrined in the sacred writings of ancient Judaism that Christians would come to call the Old Testament.

Disagreement over the meaning and divergence in the texts of that myth of origins, which marked the medieval consultations discussed by Cohen, also came early. Disputes arose not only because Christians found their Christ foreshadowed in Judaism's sacred writings while Jews did not but also because each side used different texts. "The sacred book of the entire hellenistic Jewish world . . . and the instrument of Jewish religious propaganda in the Greco-Roman world"[4] was not the Hebrew Scriptures but the Septuagint, the Greek version, which was started in the third century before the Common Era as a translation of the Pentateuch, the first five books of the Hebrew Scriptures, and was continued thereafter. Consequently, the Septuagint also became the Bible for the early Greek-speaking gentile adherents to Christianity. But "correspondence between the developments of the Hebrew and non-Hebrew versions terminates somewhere at the end of the first century A.D. By then the division between them is in fact no longer a division along linguistic lines but reflects the schism between the Synagogue and the Church and their different attitudes to the text."[5]

The first Latin version of the Old Testament for Christians was what Beryl Smalley called "an unliterary translation from the Septuagint."[6] But the Old Testament for Christians in the West during the Middle Ages was the Vulgate version, the translation that St. Jerome made in the late fourth century after learning Hebrew and consulting Jewish teachers. Yet, although based on the

[4] H. H. Ben-Sasson, ed., *A History of the Jewish People* (Cambridge, Mass., 1976), 293.
[5] P. R. Ackroyd *et al.*, eds., *The Cambridge History of the Bible*, 3 vols. (Cambridge, 1976), 1: 169.
[6] Smalley, *The Study of the Bible in the Middle Ages* (3d edn., New York, 1984), 21.

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Hebrew Bible, which Jerome called the Hebrew truth (*Hebraica veritas*), the translation was now influenced by Latinity and indissolubly linked with, and interpreted through, the New Testament, whose canon had been firmly established by the fourth century. Then, with the fall of the western Roman Empire, scholarly dialogue between Catholic Christianity and Judaism effectively ceased. Jews in Western Europe were largely cut off from the great center of Judaic thought in Babylonia, and, as a result of the new conditions and mentalities resulting from the Germanic invasions, Christian religiosity in the West from 600 to 1000 became primarily liturgical or symbolic. The theological argumentation characteristic of the Roman period almost disappeared, and surprisingly little attention was paid to the fundamental theological conflict between Judaism and Christianity.

All that changed rapidly, however, with the revolutionary social changes and intellectual developments of the eleventh century. Full knowledge of the Babylonian Talmud became available to Jews in the West, who were now a relatively defenseless minority in the larger Christian society; the work of Gershom ("the Light of the Exile"), Rashi, and others made Northern Europe the supreme center of talmudic scholarship; and Rashi's commentary on the Bible of Judaism placed a new emphasis on literal historical interpretation. Among other factors, the growing mastery of grammar, rhetoric, and logic (or dialectic) among Christian scholars not only inspired a new interest in the literal and historical meaning of the Bible but also provoked intense theological speculation, debate, and doubts about particular aspects of the faith. Then, as Berger and Cohen describe, dialogue and debate between Christians and Jews reappeared.

Berger devotes his article to the debates or polemics during the twelfth century—when toleration of Judaism had not been officially challenged and Jews could still express themselves remarkably freely—and argues that Jews actively provoked these debates. He seems to imply that such relatively free interchange came to an end in the thirteenth century because Christians realized they could not defend themselves against Jews by free intellectual debate and resolved their embarrassment by abandoning their previous toleration and pursuing missionary campaigns, backed by force, that made further open debate almost impossible.

Berger establishes the perspective for his overall argument by noting the ambivalence and tension of the Christian attitude toward conversion of Jews. For myself, while I acknowledge that Christians considered conversion of individuals desirable and welcome, I do not believe—despite some of Pope Gregory I's efforts—that there was any widespread interest in converting Jews by persuasion between 500 and 1100. In the sixth and seventh centuries, the only "conversions" of Jews in large numbers were the result of threats and force, which thereafter subsided. In the ninth century, Charlemagne and Louis the Pious seem to have respected Jews highly, and the ecclesiastics most concerned to offset that favor, Agobard and Amulo of Lyons, were far more concerned to restrict Jewish legal status and contacts between Jews and Christians than with any mission to the Jews. At the end of the eleventh century, there was no effort to persuade Jews, and a new campaign of force only demonstrated how unlikely voluntary conversion was.

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In 1096, Jews who lived surrounded by the success of Christendom faced the first major effort to convert them collectively by force since Visigothic Spain—the attacks on the Jews in the Rhineland by the popular crusaders. But they made their refusal to believe in "the hanged one" obvious by mass martyrdom.[7]

Christian indifference toward conversion of Jews by persuasion between 500 and 1100 and the major efforts to convert them in the thirteenth century frame Berger's principal point: although the dramatic multiplication of Christian polemics against Judaism in the twelfth century might seem to demonstrate a sudden interest in converting Jews, the polemics were not in fact usually intended to convert Jews either individually or collectively. Rather, they were written to defend Christianity against the explicit and even aggressive criticisms of Christian ideas and beliefs made by proud Jews. Berger argues his point convincingly, and I agree fully with his valuable contribution to our understanding of the polemics. How far such criticisms got under the skins of Christians is strikingly illustrated by an indignant outburst addressed to the king of France by Pope Innocent III in 1205.

> What is even worse, blaspheming against the name of God, they publicly insult Christians by saying that they believe in some peasant who had been hung by the Jewish people. Indeed, we do not doubt that he was hung for us, since he bore our sins in his body on the cross, but we do not acknowledge that he was rustic in manner or birth. To the contrary, not even they can deny that, according to the flesh, he was descended from priestly and royal stock and that his manners were distinguished and proper.[8]

Innocent's outburst raises a question that Berger does not really answer. Why was that powerful pope so upset by the aspersions cast on his faith by a small and quite powerless minority that had long been thought of as blinded disbelievers and deicides? The only explanation for the polemics emphasized by Berger is that Christians were reacting to "a genuine, vigorous challenge from a proud and assertive Jewish community." Berger does, indeed, hint at another explanation: the increased intellectual sophistication associated with the "twelfth-century renaissance" led to the posing of more difficult questions by Jews and a greater facility in responding to them by Christians. But that seems only a restatement of the main thesis. Is that an adequate explanation of the polemics?

On the one hand, if the article is not intended to explain the polemics but to demonstrate the existence and impact of assertive Jewish pride, there is no attempt to explain that Jewish offensive, save for the reference in note 71 to the important work of Jacob Katz. Had I been trying to demonstrate the impact of Jewish initiatives on Christian thought, I think I would have approached the subject differently. I might have started with a brief description of the impact on Jews of the reception of the Talmud and recent threatening developments in the society that surrounded them, then sketched the character of Jewish negative attitudes to Christians and Christianity, using evidence other than the polemics, such as the Toledot Jeshu and the Hebrew chronicles of the crusades. I would then have

[7] See Shlomo Eidelberg, trans. and ed., *The Jews and the Crusaders* (Madison, Wis., 1977).
[8] Solomon Grayzel, *The Church and the Jews in the XIIIth Century* (2nd edn., New York, 1966), 106.

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developed my main argument, the one Berger has made far better than I could, that the public and aggressive expression of those attitudes contributed significantly to the reappearance of Christian polemics against Jews.

On the other hand, if my primary concern had been to explain the causes and significance of the renewed Christian polemic, I would have taken a very different approach. Here, I can only suggest, but not properly support, a perspective with which Berger may agree, judging from his perceptive asides about Christian doubt and defensiveness. I would suggest that the eleventh century marked the beginning of a period in which Christians at different social levels were assailed by doubts about their identity. Awareness of the presence and disbelief of Jews was an important reason for those doubts, but it was neither the first nor the only one. Doubt led Christians to rethink their faith and rethink it in a way that would change it radically and have baneful consequences for Jews. As Richard Southern has said, this period has the air of an age of conversion.[9]

From about 1000, we can trace the beginning of a new emphasis on the humanity of Jesus, on his historical life, mother, and crucifixion, which spread throughout Europe and became dominant in the twelfth century. In 1022, Christians were burned at Orléans as heretics, the first such execution since that of Priscillian in 385. The Christians killed at Orléans apparently denied among other things that Christ was really human and that baptism, the Eucharist, and prayers for the dead were of any value. Allegedly, they refused to believe something just because it was written down and preferred to trust their natural reason. Members of other small movements in the first half of the eleventh century also doubted either the humanity or the divinity of Christ, denied the efficacy of the sacraments, and emphasized the inspiration of the Holy Spirit and evangelical preaching. At a more scholarly level, and connected with the new interest in logic and the natural world, the denial by Berengar of Tours in the 1040s that the historical body and blood of Christ were present in the Eucharist provoked many official condemnations and a widespread intellectual debate in which one issue was the relation between reason and authority. From 1050, the movement of papally led revolution known as the Investiture Contest challenged ideas about the location of religious authority and provoked a massive debate about the proper organization of Christian society. In 1095, Urban II's summons to holy war marked the beginning of a new awareness of the reality and extent of Islam and a countervailing conception of Christendom. And by the end of century the new attraction of the eremitical life, evangelical preaching, and concentration on the humanity of Jesus led to the foundation of a flood of new monastic orders that emphasized poverty and challenged the emphasis on liturgy of the earlier age. To all this may be added the unsettling impact of political reorganization and what has been described as a commercial revolution. Developments independent of Jews confronted medieval Europeans with many challenges and impelled them to debate and polemicize on many subjects.

[9] Southern, *The Making of the Middle Ages* (New Haven, Conn., 1953), 228.

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A striking example of the impact of these challenges on thoughtful Christians is the reaction of Peter the Venerable, the last great abbot of Cluny. Both Berger and Cohen discuss him because he wrote one of the largest and, in its own peculiar way, most thoughtful of the polemics against Jews. But Jews were by no means all that concerned him. About 1132, when he was about forty, Peter wrote his first little theological treatise to correct a monk who had fallen into the Apollinarian heresy.[10] Between 1130 and 1134, he intervened in the argument about the ongoing papal schism.[11] Between 1137 and 1141, he wrote his first major theological treatise, a refutation of, or polemic against, Peter of Bruys and his followers.[12] To Peter the Venerable's horror, these deviant evangelical Christians had rejected the Eucharist, infant baptism, and the authority of priests. Even worse, they rejected the Old Testament and accepted only those parts of the New that they believed were reports of eyewitnesses. Peter therefore used the authority of the New Testament to argue the authority of the Old. Then, in 1142, a visit to Spain inspired him to commission the first Latin translation of the Koran and to write his polemical treatise against Islam, whose vast extent he now recognized as a challenge to his belief in Christ's universal dominion.[13] About the same time, he wrote a little treatise to correct some of his own monks at Cluny who were remarking that Christ had never said openly in the Gospels that he was God.[14] Moreover, throughout this period, he was trying not only to repair Cluny's economic situation, which had led to borrowings from Jews and Christians, but also to defend Cluny against Cistercian criticism, and approximately an eighth in volume of his extant correspondence is devoted to a polemical defense of Cluniac monasticism.[15] Peter was reacting to challenges from many quarters that raised unsettling questions about his conception of his cosmos and himself.

Against that background, the massive polemic against Jews that Peter completed in 1144 appears as but a part, albeit a major part, of his broader and frequently polemical reaction to the diverse challenges he faced. Peter thought Jews fenced stolen goods, he advised King Louis VII to despoil them to support his crusade,[16] and he hated them to the point of questioning their humanity. But what bothered him was not their own beliefs or any serious fear that their interpretations of Scripture would be seen as authoritative by Christians or that Christians would turn to Judaism. What bothered him was the fact of their disbelief, for, like that of the Saracens, it suggested that his belief in Christ was not reasonable. Indeed, after spending most of the first four of the five chapters of his polemic on the age-old arguments about Old Testament passages, he recognized that such arguments from authority could never be decisive and turned to a surprisingly

[10] Giles Constable, ed., *The Letters of Peter the Venerable*, 2 vols. (Cambridge, Mass., 1967), 1: no. 37.
[11] *Ibid.*, no. 40.
[12] James Fearns, ed., *Petrus Venerabili contra Petrobrusianos hereticos*, Corpus christianorum, Continuatio mediaevalis, vol. 10 (Turnholt, 1968).
[13] James Kritzeck, *Peter the Venerable and Islam* (Princeton, N.J., 1964).
[14] Peter the Venerable, *Epistola ad Petrum de Joanne*, in *Patrologia Latina*, ed. J. P. Migne (hereafter, *PL*) 189: 487–508.
[15] Constable, *The Letters of Peter the Venerable*, 1: nos. 28, 111.
[16] *Ibid.*, 1: no. 130.

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different justification of the truth of his faith that bypassed the question of biblical authority.

Peter found himself forced to inquire into the very bases of true faith. These, he said, are force, voluptuousness, authority, reason, and miracle, or some combination of them. The first two, which he associated with Islam, he dismissed out of hand. By contrast, Christianity spread miraculously, although preached by people who had no force and could offer no material rewards. Peter also dismissed authority, recognizing that only those who already had faith would accept that faith's scripture and law. And, although he held reason to be the distinguishing human characteristic, he knew that it alone would not produce true faith, since the pagans of antiquity had reason but not the true faith. Only miracle, or rather the interaction of reason and miracle, could produce it. But he recognized that the reports of Christ's miracles in the New Testament could be challenged as lies or the result of deception. What then? There was, he said, one miracle that reason could not deny, since it was observed annually by both Christians and Saracens. The miraculous lighting of the lights in the Holy Sepulchre at midnight on every Easter Saturday demonstrated that Christ had arisen from the dead at that time in that place.[17] Peter's final argument to silence doubts about the truth of his faith was a miracle that Arabs had already explained as a trick of fireworks and that the pope would condemn as a pious fraud in 1238.

Toward the end of the twelfth century, Peter of Blois provides another example of the growing awareness of educated Christians that there was a disturbing diversity of challenges to their faith. Peter wrote not only the polemic against Jews discussed by Berger but also *De Fide*, his last work, a collection of definitions and arguments to rebut past heresies. And, as he explained in his preface, his purpose was to quiet his own intellectual doubts.[18] By the middle of the thirteenth century, the increasing complexity of thought had vastly increased awareness of possible doubts. Aquinas began each of the thousands of articles in his *Summa theologica* with a rehearsal of the challenges to the position he wanted to argue. And that brings us to Cohen's approach. In contrast to Berger, he argues that it was developments internal to Christianity that initiated religious dialogues between Christians and Jews, but what Jewish scholars said so threatened Christians that they withdrew from the dialogue to protect their faith by other means.

After an introductory bow to semiotics and hermeneutics, Cohen examines the twelfth-century Christian scholars who developed new techniques and consulted Jews to establish the text of the Old Testament and clarify the meaning of its passages for Christians. But the *Hebraica veritas* that they encountered endangered Christian theology. Either they had to accept the scholarly validity of Jewish interpretations of the literal meaning of certain passages in Hebrew Scripture and

[17] Peter the Venerable, *Tractatus adversus Judaeorum inveteratam duritiem*, *PL* 189: 585–601. On this surprising event, which continues to this day, see G. Klamath, *Das Karsamstagfeuer-Wunder der heiligen Grabeskirche*, Studien und Mitteilungen der kirchengeschichtliche Seminar der Universität Wien, no. 13 (Vienna, 1913); A. S. Tritton, "The Easter Fire at Jerusalem," *Journal of the Royal Asiatic Society* (1963): 249–50; and Bernard McGinn, "Iter Sancti Sepulchri: The Piety of the First Crusaders," in B. K. Lackner and K. R. Philp, eds., *Essays on Medieval Civilization* (Austin, Texas, 1978), 33–38.

[18] Richard W. Southern, *Medieval Humanism* (New York, 1970), 124.

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reject the Christian interpretations that had supported Christian theology or they had to end the dialogue in order to protect the developing, systematic, Christian understanding of the cosmos that found expression in the *summae* of the thirteenth century. They chose to end the dialogue and justified their action by condemning the Talmud, of which they had just become fully aware. As a result, Cohen concludes, the theological utility of the medieval Jew for Christians was "nearly eliminated."

In the course of his argument, Cohen provides an excellent description of the way Christian scholars reacted to their increasing realization that Judaism had continued to evolve since biblical times and was now very different from the conception of Judaism that Christians had derived from their reading of the Old Testament. I fully agree with Cohen's analysis of how medieval Christian scholars rationalized the fact of rabbinic or talmudic Judaism to suit their needs, making Christians the judges of true Judaism and depicting Jews as Judaic heretics even blinder to the meaning of their own Scriptures than they had been at the time of Christ. Also well taken is his point that, since Christian—and Jewish—scholars had no objective standard to distinguish literal from figurative or allegorical readings of Old Testament passages, and since Christian scholars erroneously assumed that Jews were only capable of literal interpretations, Christians argued with Jewish allegorical interpretations in the Talmud as if Jews had meant them literally.

What bothers me is an overtone or implication I think I detect throughout the article. Cohen concludes that dialogue did not lead to greater toleration because the Christian scholars lacked modern standards of textual criticism and fitted their reading of the Bible to "the predetermined mold of their theology." But this conclusion, while true, not only seems unsurprising and anachronistic but also seems to imply that the twelfth-century *Hebraica veritas*, which Christian scholars rejected, did not suffer from those same anachronistic limitations.

I find it surprising, especially given the initial reference to hermeneutics, that Cohen pays almost no attention to the fact that, for Christians, the Bible to which they applied their hermeneutical skills, interpreting the whole by the parts and the parts by the whole, contained another and for them ultimate testament. That is particularly strange since the eleventh and twelfth centuries were the period in which Christians began to reemphasize the New Testament, concentrate on the historical story of Jesus in the Gospels, and develop the new image of Christ as a suffering human being that accounts for so much of Francis of Assisi's vast appeal—and so much hatred of Jews.

Modern techniques of textual criticism and standards of historical objectivity were unknown, and each side considered its Bible as divine authority for its own way of life, rather than simply a subject for objective analysis. Therefore, it seems impossible to imagine that Jews and Christians could ever have agreed on their interpretations of Hebrew Scripture and the Old Testament or even on crucial passages in them. The fact that a few, remarkably few, Christian scholars, such as Andrew of St. Victor, occasionally expressed pride in their knowledge of Jewish interpretations in a way that contradicted their fundamental convictions hardly modifies the picture.

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Andrew was a glossator who concentrated not on whole texts but on particularly difficult passages in them, was concerned only with the literal or historical sense, and was no theologian.[19] But he was almost alone in this compartmentalized use of agnostic common sense. The only other twelfth-century exegete with considerable knowledge of contemporary Jewish thought whom Cohen discusses at any length is Herbert of Bosham. Yet, if Herbert welcomed Jewish exegesis when it seemed to add to Christian understanding of a text without contradicting Christian doctrine, he rejected Judaic intepretation when there was obvious disagreement and accused the Jews of distorting the Scriptures. To say that "knowledge gleaned from Jewish books remains . . . the foundation for Herbert's understanding of the Old Testament" seems a considerable exaggeration.

The evidence that consultation with Jews posed any significant threat to Christian belief in the twelfth century seems minimal. And, if that is the case, it is not surprising that Christian thinkers in the thirteenth century continued to use their knowledge of contemporary Jewish ideas in a way that distorted them but served to rationalize the Christian faith. For Christians, like Jews, faith did not spring from intellectual argument or academic truth. It derived its strength from consciousness of a very different kind, from the historically conditioned, nonrational way these human beings symbolically integrated their social and individual needs, experiences, memories, emotions, and ideas to find meaning and value in their universe and to place themselves within it.

That Cohen has not always taken into account the basic mentality of Christian thinkers is indicated by several expressions. To say, as he does in his introduction, that "Christianity had always acknowledged its debt to the Old Testament" is a peculiar way of putting it, since for Christians the Old Testament was an integral part of what their God had revealed for their salvation and its Psalms were central in their liturgy. It also seems misleading to suggest that because Christian biblical commentators believed that Jews observed the literal teachings of the Old Testament, and because they used information from contemporary rabbis about the meaning of specific passages, they therefore thought that Jews best understood the "primary meaning and significance" of the Old Testament. And it seems somewhat one-sided to say that, up to the twelfth century, Jews were perceived as spokesmen of the Old Testament who were tolerated because they "served to convey *the meaning* of their Scriptures to the Christians" (my emphasis). At the least, that assertion needs to be balanced by reference to the other Christian explanation expounded from Augustine forward: Christians should tolerate Jews because, although the Scriptures really belong to the Christians, the Jews do read them blindly and venerate them and thereby they serve to prove that the Christians did not forge the prophecies about Christ.

AS IS, I HOPE, APPARENT, so far as I disagree with Berger and Cohen, it is not over the value of their enterprise or of the facts they establish; it is because of a difference in approach and interpretation. And that reflects a difference in the

[19] Smalley, *The Study of the Bible*, 120–21.

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questions we would ask. Berger is concerned to establish that autonomous action of Jews caused the Christian polemics; I would ask why the human beings on either side acted as they did. Cohen is concerned to demonstrate that increased knowledge of contemporary Judaism did not make Christian scholars and ecclesiastics more objective and tolerant but rather the reverse; I would ask how the way those human beings developed their consciousness of identity determined the use to which they put their discoveries. Since my fundamental question as a historian would be why these Europeans came to hate each other as much as they did, I would not think of them primarily as they were defined by the ideas of their Judaism or Catholicism. I would think of them as human beings who had common basic characteristics but who were highly unequal in social power, who had overlapping but distinctively different cultures, who were defining themselves ethnocentrically in a rapidly changing environment, and who, precisely because of their cultural and physical proximity, were making their differences a central element of their identity.

From that perspective, and if we leave highly influential social, economic, and political developments aside and focus on religious thought, two important causes of the increasing mutual hostility seem to stand out. The reception and implementation of the Talmud (which had been formulated in a vastly different society) and the pressure of increasing Christian hostility and exploitation radically reinforced the Jews' sense of a separate and highly distinctive identity. And the new awareness of, and emphasis on, the historical dimension of the New Testament radically sharpened the Christians' sense of their distinctive identity at a time when they increasingly felt that they were the masters of their environment and when their leaders felt increasingly able to demand that people had to conform to their conception of what a Christian society and the Jews in it should be. But it was not religious ideas alone that enabled Christians to disregard the reality of Jews and become increasingly irrational in their hostility toward them. Brute force was necessary. Disparity in power encouraged Christians to exploit Jews, repress autonomous Jewish self-expression, and set the self-fulfilling prophecy in motion. For, by that process, Christians were able to control Jews in many ways and, by torture if necessary, make them seem living proof of the eternal value of the beliefs about Christ by which Christian Europeans symbolically coordinated their sense of who they were. Yet, although these non-Jews made Christ symbolically central in their identity, how different was their consciousness of identity from that of Jesus of Nazareth.

GAVIN I. LANGMUIR
Stanford University

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Reviews of Books

GENERAL

MIROSLAV HROCH. *Social Preconditions of National Revival in Europe: A Comparative Analysis of the Social Composition of Patriotic Groups among the Smaller European Nations*. Translated by BEN FOWKES. New York: Cambridge University Press. 1985. Pp. xiii, 220. $44.50.

Miroslav Hroch has reworked his seminal study of nation-formation among smaller European nations, which was first published in 1968. In this new English version he has clarified and expanded several points, added data for the Danish minority in Schleswig, and taken into account much scholarship that has appeared since 1968. For these reasons, this new version supplants the earlier edition. Since Hroch's study has already achieved the status of a classic, I would like to raise some questions about his typology rather than to give his careful, thorough research and analysis the praise it deserves. During the following, one should keep in mind that Hroch's book remains today what it was in 1968: an original, detailed, scholarly, and important work.

Hroch is not dogmatic; in fact, he is modest and subtle. But, beginning with a firm faith that the development of exchange relations and a national market constitutes the "decisive precondition for the formation of a modern nation" (p. 5), he finds by the end of the book that the growth of a national consciousness occurred primarily in the most productive and market-oriented regions of the small nations he studies, and he suggests that the petty bourgeoisie was involved in this process in a prefiguring way. He concludes that the growth of communication and social mobility that was fundamental to the development of national consciousness was "not a primary factor in social development, as some Western scholars imagine, but a reflection of the implantation of new forms of production and a new, capitalist type of relation" (p. 191). This formulates a major part of Hroch's case with some crudity, but I do not think it misstates the author's fundamental belief that economic and social ingredients, not political or ideological ones, determined the course of nation-formation in small European countries.

The adjective Hroch assigns to the eight ethnic groups he studies is "oppressed," by which he means that these small peoples had incomplete social structures at the beginning of the modern era owing to long subjection to a ruling class (p. 9). He suggests that the normal, integrated way national consciousness arrived to these peoples was when national agitation began at the same time as the Industrial Revolution and was followed by the introduction of liberal constitutionalism, which Hroch calls the bourgeois revolution. If, however, national agitation began only after the coming of the Industrial Revolution and the bourgeois revolution, then mass nationalism did not occur. Nation-formation fails in an ethnic minority if it begins when liberal constitutional guarantees are already in effect.

But why should this be? If nation-formation is at root a product of capitalist relations, it should not make a fundamental difference whether the national agitation begins before or after the introduction of political reforms, if, in fact, it can begin at all before the coming of the Industrial Revolution, which Hroch admits it can. What Hroch's typology actually suggests is that the independent variable in nation-formation is not social but political relations. Although oppression certainly takes socioeconomic forms, it is essentially a political category, referring to exclusion from the benefits of political participation. When liberal constitutional reforms make the adequate articulation of individual and group needs possible, nationalist agitation may be less necessary to the achievement of the senses of autonomy and community that nationalism provides.

The term "oppressed" raises other questions as well, since it is a relative term. Traditional modes of living accepted by all become recognized as dominance and oppression only when new ideas about social organization present themselves. In a sense, Hroch's book is about the question of how small peoples learned they were oppressed. For Hroch, consciousness is a surface manifestation, and ideology is effective only when it reflects interests. But

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how, then, do the first writers and intellectuals come to realize that their nation is oppressed when, in fact, no economic or social change has taken place? After demonstrating clearly that no single class or stratum can be identified as present in all of his cases of national development, Hroch answers this question by claiming that the petty bourgeoisie, carters, farmers, craftsmen, and so forth became "one of the main prospective vehicles of the creation of national consciousness" (p. 137), even though they had relatively small direct impact. They acted as a funnel leading to the central role of the bourgeoisie in the mass phase of national development. Further investigation, Hroch believes, would show that even the earliest intellectual awakenings had social roots. A more balanced solution, it seems to me, is simply to admit that the idea of equity (that is, the promise of the French Revolution), was sufficiently strong in its own right that some who understood it realized that what had earlier seemed like justifiable social hierarchy now seemed like oppression. Hroch's tortured effort to find in the bourgeoisie the required social agent for propogating ideology and consciousness, when his own analysis shows that no such class existed until very late in the process, suggests that the old-fashioned view that ideas have their own life and impact is at least as plausible, and a good deal more parsimonious, than an explanation that is purely socially driven.

The bulk of Hroch's book is not typology but consists of detailed studies of eight numerically small peoples during the developmental period (phase B) between the initial national awareness of a handful of intellectuals (phase A) and the emergence of full-blown nationalism (phase C—one could argue about the dates Hroch choses for the beginning of this last phase). Hroch's findings provide a useful complement to Ernest Gellner's *Nations and Nationalism* (1983), although Hroch would certainly reject many of Gellner's views. Gellner contends that nationalism is the logically necessary ideology of industrialism because it provides the ideas through which the educational system creates the culturally interchangeable individuals required by large-scale economic operations. Hroch also believes that nationalism is a necessary result under certain developmental conditions, but he would consider Gellner's point that co-culturals are produced by the educational system as too ideological because it suggests the autonomy of ideas. And yet his analysis of the groups involved in creating and propagating national consciousness provides historical examples of just how the process Gellner suggests but does not document actually worked. Comparison with other contemporary social historians and theorists of nationalism could be adduced. They would confirm, as I do, that, even if one is not convinced by Hroch's determinism, his study of nationalism remains one of the most important of the postwar era.

GALE STOKES
Rice University

LOUIS L. SNYDER. *Global Mini-Nationalisms: Autonomy or Independence.* (Contributions in Political Science, number 71; Global Perspectives in History and Politics, number 2.) Westport, Conn.: Greenwood. 1982. Pp. xvii, 326. $29.95.

LOUIS L. SNYDER. *Macro-Nationalisms: A History of the Pan-Movements.* (Contributions in Political Science, number 112; Global Perspectives in History and Politics.) Westport, Conn.: Greenwood. 1984. Pp. xii, 308. $35.00.

During his long professional career Louis L. Snyder has published dozens of books, many of them on nationalism, which he justly identifies as one of the most important historical forces of the contemporary era. But, although he pays homage to the authoritative work of C. J. H. Hayes, Hans Kohn, and Boyd Shafer, Snyder laments that most scholarly work on nationalism has been almost exclusively focused on traditional forms of the phenomenon as manifested, for example, in France, Germany, and Russia. Thus, little work has been done on their so-called corollaries, which he labels "mini-" and "macro-nationalism." By the first, Snyder means liberating movements of minorities inside centralized states who demand either more autonomy in local affairs or outright independence. By contrast, he describes macro-nationalism as broader pan-nationalist movements that seek to combine similar nationalisms in a larger movement: pan-Slavism, pan-Germanism, pan-Arabism, pan-Africanism, even pan-Americanism and pan-Europeanism.

To fill this gap in the scholarly literature Snyder decided several years ago to write a one-volume comparative analysis of mini- and macro-nationalisms. But, overwhelmed by the mass of material that confronted him, he soon concluded that such a study went beyond the scope of a single book. Thus, he split it in two and elected to publish separate volumes on each phenomenon, the one on mini-nationalism appearing in 1982 and the other in 1984.

This was an unfortunate decision, for in its wake the comparative and analytical focus of Snyder's original project was lost (though, given his oft-expressed hostility for history as a scientific discipline, one may legitimately wonder if he was intellectually and temperamentally suited to the task in the first place). In any event, Snyder at length produced two compendium-style volumes in which he presents little more than a heavily anecdotal, journalistic, and ultimately superficial survey of dis-

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sident minority nationalisms on the one hand and pannational movements on the other. And along the way he indulges his rather too evident distaste for Marxian analysis, the Soviet Union, and anything else that smacks of radicalism, be it the PLO, the IRA, or Khomeini's regime in Iran.

But what finally unites these books is Snyder's common aversion for both varieties of nationalism that he is studying. The "mini" version he faults for being suffused with irrationalism, emotion, and authoritarianism and for usually having as leaders violent men with a penchant for terrorism for its own sake. This, of course, is to misunderstand completely the situation of the minority nationalist who, more often than not, becomes an extremist not for reasons of personal choice but out of necessity, confronted as he is by an unyielding centralized state. The formula is simple, even if Snyder cannot see it: unreasoning intransigence on the one side breeding frustrated extremism on the other.

Thus, Snyder treats few of the mini-nationalisms he surveys with sympathy, except for the Greenlander, which is arguably the least significant, and the Kurdish, which he obviously admires more for the enemies it provokes—Turks, Marxists, radical Iraq, Khomeini's Iran—than for the goals it seeks. Moreover, although he recognizes that mini-nationalisms are probably an inevitable and enduring consequence of the incompatibility between the absolute right of state sovereignty and the equally powerful claim of self-determination, Snyder considers them of no more than marginal historical significance. They are simply too weak and internally divided between moderates and extremists to resist successfully the centralized states that they oppose—a curious conclusion given the considerable victories that Bretons, Corsicans, Alsatians, Tyrolese, Sicilians, and Flemings have scored in recent years against their respective central governments. Snyder does acknowledge one exception, and that predictably enough is the Soviet Union, whose leaders face—he argues with a favorable reference to the hardly disinterested Zbigniew Brzezinski—centrifugal pressures even more threatening than the racial problem in the United States. It need hardly be said that with this statement Snyder abandons the terrain of dispassionate scholarly analysis for the more comforting one of ideologically charged hope.

As for the macro-nationalisms, Snyder dismisses them collectively for having been uniformly unsuccessful in achieving their goals. Moreover, they have typically been promoted by men who are either visionaries or cranks. Thus, condemned to political isolation and deprived of any real influence, they and the movements they lead have been little feared and easily tolerated by governments that regard them as no more than nuisances. However true this may be of many of the panmovements that Snyder examines, it is a gross underestimation of powerful currents that flow in contemporary Western Europe in the direction of supranational economic, political, and cultural forms.

In the final analysis both Snyder's volumes are unsatisfying because they consistently underestimate and devalue the phenomena they purport to study. This is especially the case insofar as contemporary Western Europe is concerned. Since 1945 disintegrative minority nationalisms on the one hand and surging movements toward supranational European institutions on the other have dramatically undermined the authority of existing nation-states to the point that their long-term prospects for survival have been rendered problematical. This discomforts Snyder, for he admires much about these old states and sees in the various nationalisms with which they are associated a "collective expression of the desire for freedom" (*Global Mini-Nationalisms* [p. 7]). This is a claim that virtually all minority nationalists in contemporary Europe denounce as either absurd or false, while pan-Europeanists reject it as irrelevant or at best as misleading. Snyder can understand neither, for he has taken his stand with the past. He therefore cannot see the Europe of the future, whose emerging shape will almost certainly be determined by the work of mini- and macro-nationalists alike.

JACK E. REECE
University of Pennsylvania

WILLIS B. GLOVER. *Biblical Origins of Modern Secular Culture: An Essay in the Interpretation of Western History*. Macon, Ga.: Mercer University Press. 1984. Pp. x, 286. $23.95.

Books by intellectual historians on the religious situation of the modern Western world are somewhat rarer than a few decades ago, but welcome nonetheless. Willis B. Glover begins his "essay" with a familiar critique of twentieth-century nihilism in the vein of C. S. Lewis and then, happily, devotes most of his energies to a generally dispassionate exploration of the Christian sources of Western secular thought. Although the author has little sympathy with the various humanisms that have from time to time displaced biblical faith in so many prideful modern hearts, his central concern is to show that each is rooted in the faith it rejects. "What is distinctively Western," he affirms, "will be seen again and again to result from the actualizing in history of cultural implications in the biblical tradition" (p. 15).

Such sweeping declarations are impossible to prove, as is the contrary view that modernity constitutes a wholesale rejection of traditional belief. But Glover argues his case fair-mindedly, taking predict-

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able issue with the likes of J. B. Bury and Peter Gay and drawing aid and comfort from Carl Becker and Charles Trinkaus, among others. He also follows Paul Hazard in identifying the late seventeenth and early eighteenth centuries as the age of crisis in European consciousness, the time when modern man first cast off the anchor of faith in a transcendent, omnipotent God and assigned to himself the unlimited freedom once reserved to the deity. The humanism of the Enlightenment, although still deeply indebted to Christian values and concepts, radically transvalued the place of man in a cosmos of which he was now the self-appointed master. The new humanism, writes Glover, took its place beside Judaism, Christianity, and Islam as "a fourth biblical religion" (p. 108).

Other humanisms came later, including the Promethean humanism of the Romantics, the scientific humanism of Henri de Saint-Simon and Auguste Comte, Marxism, and existentialism. All have had their champions, but Glover finds that all have suffered from grave internal contradictions, such as the antithesis between a deterministic view of nature and a voluntaristic image of man. One outcome is the sense of meaninglessness and despair that afflicts many thinkers of the twentieth century. Along the way Glover takes time to question the debt of Renaissance culture to paganism, fault modern theology for its perversion of Kantian dualism, and attribute Western civilization's unique fascination with history to the biblical heritage.

This is a large book, recalling in many ways Franklin L. Baumer's magisterial *Religion and the Rise of Scepticism* (1960), which, surprisingly, it fails to cite. Most of the ground traversed is not new. Relatively few scholarly works published since 1970 were consulted, giving the book a rather musty scent. But the themes it engages are so important, and its arguments are presented with such clarity and reasonableness, that almost any student of modern thought will find something of value in its pages.

W. WARREN WAGAR
State University of New York, Binghamton

MARK POSTER. *Foucault, Marxism, and History: Mode of Production versus Mode of Information*. New York: Basil Blackwell or Polity, Cambridge, England. 1984. Pp. 173. Cloth $29.95, paper $9.95.

This book by Mark Poster is not a standard intellectual history but an extremely ambitious attempt to intervene in the current critical scene and to provide a basis for the author's forthcoming attempt to furnish a radically transformed version of critical theory. Although judgment must to some extent await that future contribution, this book provides food for thought and anticipates Poster's future themes. It may thus be compared to Perry Anderson's *Considerations on Western Marxism* (1976), although its incentive is not so much to renew the basis for Marxist theory and practice as to show why Marxism itself must be transcended in the direction of a critical theory more alert to contemporary problems and possibilities.

Poster's discussion is informed and thought-provoking, even if one disagrees with certain emphases and arguments. He provides a lively treatment of the perceived defects of classical Marxism that moved a variety of theorists to look to other theoretical tools. Poster observes that Foucault takes up the challenge of social history, turning from texts written by elites to the "mundane discourses of disciplinary institutions that more directly affect the everyday life of the masses" (p. 87). But Foucault shocks the sensibilities of social historians. The objective of this seemingly "anti-historical historian" (p. 70) is not to furnish an empirically accurate reconstitution of past social contexts in their own terms and for their own sakes but to show how this contemplative ideal of scientific objectivity itself must be situated in a matrix of power and knowledge and displaced in favor of historiography in a different key—one more sensitive to the discontinuities of the past and open to the insistently acknowledged political demands of the present. This kind of historiography resists "totalizing" frameworks, whether they are inspired by the Marxist desire for revolution or by the more anodyne methodological quest for *l'histoire totale*.

Poster's major thematic claim is that the decline in relevance of Marxism and the promise of Foucault are related to the change from an economy based on labor and production to one based on the "mode of information." Poster defends the latter concept on the basis of the shift from the primary (agricultural) and the secondary (industrial) sectors to that of the tertiary (service) sector, in which one has not the dominance of people working on nature or things but "people acting on information and information acting on people" (p. 53). In this setting, one witnesses a "social field pervaded by linguistically rich forms of action," including "new forms of social interaction based on electronic communications devices" (p. 168). This shift in the economy and society grounds Foucault's turn to the role of technologies of power and domination in discourse and practices.

The obvious question is whether we are indeed amid such a shift from an older mode of production to a newer mode of information that would underwrite the lessened relevance of Marxism and promote Foucault as guide in the elaboration of a radically new critical theory. This question cannot be convincingly addressed solely on the basis of the relativistic perspectivism in which Poster seems to

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confide. Throughout the book, Poster qualifies Foucault's theses by arguing for the relation between the penal and the industrial systems, the need for closer class analysis in the history of sexuality, and the importance of resistance to structures of domination. He even argues that a "moment of totalization" is "necessary and desirable" to situate self-reflexively the theorist's own discourse and to indicate its place in the social field. These are, however, also qualifications of his own thesis. They would seem to indicate that the task is to articulate better the relation between Marxist and Foucauldian analyses, especially with respect to the role of signifying practices, the interaction between collective discourses and specific events, and the bonds, animosities, or modes of indifference connecting workers to other oppressed groups.

Near the very end of the book, Poster acknowledges that "society could not continue to exist without the uninterrupted production of commodities" and that "questions of nuclear war and ecological balance with nature must remain the top priorities of critical inquiry for obvious reasons" (pp. 167–68). The apparent problem Poster will have to confront in his follow-up study is how to develop more fully the relation between these issues and the so-called mode of information, a problem that requires a less resounding emphasis on the epochal importance of the mode of information and a more modulated idea of the necessary modification of critical theory. The sharply etched and provocative arguments of this book will nonetheless provide a fruitful incentive for any future attempt to rethink the grounds of critical theory.

DOMINICK LACAPRA
Cornell University

PETER GAY. *The Bourgeois Experience: Victoria to Freud.* Volume 1, *Education of the Senses.* New York: Oxford University Press. 1984. Pp. 534. $25.00.

With this sprawling study Peter Gay does nothing to define nineteenth-century bourgeois sex historically—to distinguish it from sex in other eras and classes. But he does document it richly and faithfully, minor shortcomings aside. The worst of these is that his coverage is skewed to marital and genital sex and that his sources are monotonously American, English, French, and German; his sole token of Spain is a medical text freely rendered into French (p. 431), and of Italy "La donna è mobile" misspelled (p. 432).

And his interpretations? His proudest finding is no news: that some Victorians enjoyed sex (even in wedlock) whereas others did not. His key exhibit is one Mabel Todd out of Yale's archives, who tirelessly—and tiresomely—recorded her fun at sex with her husband and, later, with her lover. Following up this "eloquent clue" to his subject (p. 16), Gay culls sexy passages galore from other private records of the day. Actually, some women of Mabel Todd's vintage publicized their erotic joys for themselves: witness Lou Salomé. Yet Gay exposes his enjoyers gloatingly, with a wink at dutiful wives who found that compliance in bed "brought rewards, not all of them purely spiritual" (p. 141).

Other themes of Gay's are less well grounded. He makes much, in particular, of nineteenth-century bourgeois anxieties and insecurities with no better evidence than a quotation from Théophile Gautier about "the bourgeois himself" looking awkward at Napoleon III's court (p. 57). He attributes such malaise to "cultural change" (p. 459), which is hardly a class-specific novelty. He extends it to cover "man's fear of woman" (p. 168), including "man's anxious need to abase her lest she overwhelm him" (p. 434). As "symptoms of anxiety," he counts even "sophistries" (p. 60) and "bad science" (p. 312). Relevant by contrast is his hunch that contraception spread fastest among the bourgeois because they most feared fatal pregnancies (pp. 272–74).

These and disparate lesser contentions of Gay's—that pornography is childish, that Gladstone construed death religiously, that Victorians confronted the body matter-of-factly at times and at times not—add up to no argument. His trouble focusing may result from his larger subject, "the bourgeois experience," on which he announces more volumes to come. This experience is a false entity, as Gay himself allows, only not because experience is all necessarily individual, as he misunderstands Freud to say (pp. 15–16, 464), but because there was no delimited, self-aware bourgeoisie to have experience in common. Nor can Gay retrospectively delimit his bourgeois on the basis of *mentalité*: "bourgeois tried to live decently, educate their children, decorate their houses, and leave property" is, alas, his best try (p. 24). For want of an overall argument, his book lacks structure. Its aimlessness contrasts with his sure handling of most of the cultural material he uses. Rarely does he misread as badly as he does Frank Wedekind's *Spring Awakening*, whose youngsters, far from being "in the dark" about sex (p. 289), are, with one tragic exception, all too savvy.

Gay's manner is talky. At its worst his diction ranges from the trite to the bizarre—from "painfully clear" and "a sea of poverty" in a single sentence (p. 24) to "the touch—always touch" (p. 439)—by way of fancy nonsense on the order of "bliss" that "could achieve an allusive . . . freedom" (p. 124). Literary lapses abound such as the "middle classes" and "its critics" (p. 35), "a very army" (p. 42), "defenses from anxiety" (p. 51), "thinking on" (p. 101), the "heart" of a "shudder" (p. 102), "voices . . . who shared Droz's conviction" (pp. 149–50), or

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"Rossetti's women, for one" (p. 202). Foreign languages also suffer some, what with "Do it, say, pig!" from the French (p. 147), "pushing after" from the German (p. 273), and middle classes allegedly only an English plural (p. 44). Gay's prose may be less flawed than most, but it is no high style transmuting contents.

The one fixed line of Gay's desultory approach is Freudian. For Gay, Freud could do no wrong—or, if he did, as in calling repression "hypocrisy," he was just "too busy" to straighten out his language (p. 418). Gay continually quotes, paraphrases, rehashes, and reechoes the master. Yet, as a rule, he uses psychoanalysis only to label attitudes "oral" or "oedipal," to pick symbols out of dreams, and, above all, to hint at meanings beyond his reach. In Victorian housewifely literature "avowed principles screened unconscious conflicts" (p. 157). Mabel Todd "was in the grip of unresolved conflicts" before her marriage (p. 76), and, when her husband later connived at her amour with a friend of his, Gay begs off from exploring any man-to-man motivation: "Such matters are obscure" (p. 95). Gay's dictum that Victorian "emotional victims . . . were all made within the family" (p. 445) is Freudian dogma; his quip that, if a doctor harped on masturbation in writing about sexual ills, "the matter was much on his mind" (p. 306) is Freudian smartness; his sense that the words "How dear you were to me!" in a suicide note "insist on separating love from sensuality" is Freudian excess (p. 117). His few bold interpretive ventures are of the same Freudian inspiration. Men opposed the woman's movement? "The little boy concealed in the nineteenth-century man looked up at his powerful unpredictable mother and was afraid" (p. 192). Or again, doctors saw masturbation as inducing impotence until "the new medicine" rescued them from professional impotence (pp. 315–18). Such is the "history informed by psychoanalysis" that Gay does well to distinguish from psychohistory (p. 8).

RUDOLPH BINION
Brandeis University

OLIVIER ZUNZ, editor. *Reliving the Past: The Worlds of Social History*. Chapel Hill: University of North Carolina Press. 1985. Pp. 334. Cloth $29.00, paper $9.95.

Social history has become one of the dominant areas of scholarly inquiry during the past twenty-five years. Partly as the result of its popularity, it is now even more difficult to agree on what constitutes social history or to keep abreast of recent developments in this rapidly growing field. In an effort to assess some of the recent scholarship as well as to provide guidance for future endeavors, five prominent scholars have reflected on the state of social history in their areas of research in this collection of essays edited by Olivier Zunz. Charles Tilly considers recent scholarship in European social history, Zunz analyzes American social history, William B. Taylor examines early Latin American social history, David W. Cohen reflects on African social history, and William T. Rowe summarizes and comments on modern Chinese social history.

The essays reflect the diversity of approaches to social history today. Despite the efforts by some of the authors, especially Tilly, to establish a clearer and more concise agenda, there is little consensus even among these authors on what social history is or where future research efforts should be directed. The most explicit and direct prescription for the tasks of social history comes from Tilly, who calls on scholars to connect the experiences of ordinary individuals to large-scale structural changes such as the rise of nation-states and the development of capitalism. Although several of the other authors acknowledge the validity and importance of Tilly's suggestion, their own essays reveal a much broader and less focused approach to social history. Furthermore, Cohen even casts doubt on the wisdom of such an undertaking, because it may lead historians to miss the importance of the internal and local dynamics of African social life by directing attention mainly to external and large-scale changes initiated by Europeans.

If these historians cannot agree on a single definition or agenda for social history, they do seem to concur on the need for connecting studies of ordinary individuals and local areas to broader developments. Indeed, the plea for a more synthetic approach to the study of social history runs through all of these essays and points to the growing maturity of the field as well as to the disillusionment of many scholars with the narrow empiricism inherent in much of the work on social history today. Several authors provide some helpful suggestions on how and where to make such connections, but, unfortunately, there is relatively little discussion about the methodological problems involved in trying to link micro- and macro-level analyses.

One of the major contributions of this set of essays is to provide the reader with a critical summary and evaluation of studies analyzing social history in different geographic areas of the world. The decision to include essays on such diverse regions was a good one, since the essays on Latin America, Africa, and China point to the limitations of using categories and models developed mainly from the experiences of the United States and Western Europe. Naturally, not all aspects of social history can be thoroughly covered in such short essays. Studies of demographic and educational development, for example, are not extensively discussed by any of the authors. Nevertheless, most of these essays provide

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a good introduction to and appraisal of the most recent works in the field of social history. In this regard, the essays of Zunz, Taylor, and Rowe are particularly helpful, whereas those of Tilly and Cohen are less so because they are more focused on where the authors think that social history should be directed in the future.

Overall, this is a useful set of essays on the state of social history today. Many practitioners in this field will undoubtedly quarrel with some of the interpretations and judgments put forth, but most will agree on the value of undertaking such an ambitious and difficult endeavor. Although these essays probably will not persuade many scholars to abandon their own particular definition of social history, it is to be hoped that they will stimulate all of us to reconsider our own research priorities and strategies from a more comparative perspective.

MARIS A. VINOVSKIS
University of Michigan

HANS MEDICK and DAVID WARREN SABEAN, editors. *Interest and Emotion: Essays on the Study of Family and Kinship*. New York: Cambridge University Press or Maison des Sciences de l'Homme, Paris. 1984. Pp. xi, 417. $59.50.

Edward Shorter's suggestion that "a surge of sentiment . . . helped to dislodge the traditional family" has proved seminal in a manner that its author could not have foreseen (*The Making of the Modern Family* [1975], p. 5). It has inspired research on the family and encouraged eminent scholars from Michel Foucault to Lawrence Stone and Steven Ozment to view positive human emotions as an identifying characteristic of the modern family, a quality that distinguishes it from its traditional forebear. *Interest and Emotion* is a provocative challenge to such scholarship and a stimulating attempt to chart new directions for research. It criticizes what the editors in their prefatory essay refer to as "the way the relationship between emotional needs and material interest is handled in both the discipline of social history and anthropology . . . emotions and interests are often treated as opposites which cancel each other out" (p. 10). What follows, a collection of fifteen diverse essays resulting from a round table involving historians and anthropologists who met in Paris in 1980, attempts to explore affective and material aspects of family life. As the editors put it, "The problem is to grasp and to analyze both elements reciprocally in order to understand the forces which shape inner-familial relationships, determine the level upon which exchange takes place, and create conflict and tensions which need to be resolved or which bring about severance of connections" (p. 13).

Although the book as a whole is well organized around the central theme, its various parts are difficult to characterize. The authors are a cosmopolitan group of scholars, and their works display extraordinary range. Bernard Vernier contributes an article on property transfers and social reproduction on the Greek island of Karpathos. Regina Schulte and Vanessa Maher examine maternity and maternal feelings in nineteenth-century Bavaria and twentieth-century Morocco respectively. Articles by Martine Segalen, Alain Collomp, and David Sabean examine Jack Goody's notion that inheritance is a means by which both the social system is reproduced and personal relationships are mediated. The connections between familial strategies and political activities are examined in essays by Andrew Strathern and Roger Sablonier. Family strategies for personal and material survival are the topics addressed by Herbert Gutman and Esther Goody in their essays on Afro-American slaves and their descendants and on modern West Africans. Karen Sacks and Louise Tilly study how family values affect work routines and class values among black female hospital employees and French linen weavers respectively. In the final section of the book, essays by Hans Medick, Gerald Sider, and Karen Hausen examine social ritual and symbol, revealing ways in which social customs, rooted in domestic life, both reflect and distort the reality of the family, simultaneously liberating the participants and reaffirming the status quo.

In a brief review it is impossible to examine each essay critically or to portray their richness of method and theory. Clearly, some readers will find individual essays or assumptions more problematic than others. For example, Schulte's work on infanticide and maternal instincts in nineteenth-century Bavaria raises fundamental questions regarding the interpretation of documents. Do the feelings of a mother for her child, as reflected in cases of infanticide, correspond to the social norm? Are maternal instincts and emotions always mediated by the mother's ability to work and her access to property? Although some readers will respond to the first question negatively, Schulte's essay, like all the others, forces a reevaluation of traditional assumptions regarding the affective nature of family life. Some readers may bridle at Sabean's bold assertion that "there is no such thing as a pure unmediated attachment between individuals" (p. 171). None, on the other hand, will want to deny the correctness of his charge to delineate how property shapes personal relationships.

The most important objection that may be raised regarding the book is the tacit assumption on the part of all contributors that interest is not only connected to emotion but ultimately determines emotion. If one rejects the Marxist assumption that

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work and property are fundamental determinants of social structure and personal behavior, then one may admit the possible existence of emotions independent of interest. If one rejects any necessary connection between interest and emotion, then one may distinguish those emotions based on conflict and calculations from more positive affective ties that seem to be the object of study in "traditional" scholarship on the family.

Medick, Sabean, and company overcome these objections by demonstrating convincingly that their conceptions of the interplay between interest and emotion correspond to the nexus of obligations, responsibilities, and feelings that characterized the premodern family and still obtain to an extent in the modern one. Their essays recall scholarship on the family from the fallacies of Whig history, which, by insisting that the past bears comparison to the present, commits at once the errors of anachronism and chauvinism.

This collection might be improved in a number of small ways. The readings might be rendered less dense through the avoidance of unnecessary jargon. Furthermore, a more ambitious introduction might blend the various essays together and suggest the significance of their methods and findings. Suggestions of improvement aside, however, this is a superb and valuable collection that should inspire a reconsideration of preconceived, stereotypical notions of the family. It is essential reading for all scholars interested in the field.

THOMAS MAX SAFLEY
Wabash College

DANIEL J. KEVLES. *In the Name of Eugenics: Genetics and the Uses of Human Heredity*. New York: Alfred A. Knopf. 1985. Pp. x, 426. $22.95.

Francis Galton (1822–1911) estabished the science, or the philosophical hope, of eugenics. As a Victorian modernist, he saw in Darwinism, statistical analysis, and racial pride the means to improve the human condition. Eugenics was the social policy in which the better classes increased and the lower orders were discouraged from increasing their numbers. Since democracy and socialism protected the lower orders from their assumed Darwinian fate, Galton believed that the eugenic enterprise was a cultural imperative to overcome the emerging degeneration inherent in modern life.

Scholars such as Mark Haller, Charles E. Rosenberg, and Garland Allen have written on various aspects of the fascinating and complex history of the eugenic ideal. Daniel J. Kevles, however, provides the first "comparative history of eugenics in the United States and Britain from the late nineteenth century to the present day, giving attention to its expressions elsewhere, especially in Germany, insofar as they affected Anglo-American developments" (p. x). Kevles provides a brilliant synthesis of published and archival materials. His bibliographical essay is first-rate. From the first chapter, on Galton, to the final one, "The Songs of Deicide," the book traces the rise and fall of Galtonian eugenics, the abortive attempts of a reform eugenics, and the recent fad of sociobiology and varieties of medical techniques in human reproduction. As the subtitle of this book indicates, genetics, truly a twentieth-century science, in time replaced eugenics just as the Victorian gentleman-scholars such as Galton gave way to the scientists of genetics based in universities and research institutes. Reform eugenics after the 1930s was doomed to failure: "Human genetic research may have been spurred in part by reform-eugenic goals, but the more that was revealed about the complexity of heredity in human beings, the less did eugenics—even much of the reform variety—appear defensible in principle, or even scientifically within reach" (p. 251).

It is exactly at this point that a conceptual flaw is revealed. If Darwinism was the major socio-intellectual divide of the last century, then totalitarianism has the same status for twentieth-century history. The totalitarian presence is noted but not explored to the degree that it deserves. Of course, a fuller discussion of totalitarianism might have greatly increased the book's length; like any historian, the author undoubtedly had to determine what to emphasize.

Kevles effectively details how the "scientific" basis of eugenics and racism changed. As I. W. Newby's work reveals, the "social" element in the two creeds was no less important. Author of the prize-winning book *The Physicists*, Kevles is comfortable explaining the development of scientific thought. His historical account of genetics is quite good. For example, he argues that the synthesis of Mendelianism and population genetics was the scientific equivalent of the way in which the social values of the holders of the Galton professorship in eugenics at the University of London changed as the men moved away philosophically from Galton's original vision.

Among its many virtues is the book's clear presentation of sociobiology and its good guide to the literature on the subject. The shade of Sir Francis moves in Edward O. Wilson's analysis, because Wilson's books represent only the most recent effort to find an essence to human nature that would provide a scientific basis for social policies.

As a major contribution to the historiography of eugenics and related topics, Kevles's study gives rise to reflection on the irony that is human history. Galton and his followers were uneasy about democracy, mass culture, and the future. At different times they appealed to "the state," "the race," or "the

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nation" to redeem itself biologically from a degenerative future via collective means (such as law and social pressure) by becoming hereditarily improved and thereby ethically and morally better. Kevles recognizes the irony: "But in fact, the more masterful the genetic sciences have become, the more they have corroded the authority of moral custom in medical and reproductive behavior" (p. 301). Regardless of scientific experts, laws, and social philosophers, contemporary Americans still must exercise individual judgment in directing their personal behavior, which collectively have wide cultural consequences. What model is modern man to use as he creates himself and his world? *In the Name of Eugenics* provides an excellent guide to previous efforts; the future remains open and unknown.

DONALD K. PICKENS
North Texas State University

EVIATAR ZERUBAVEL. *The Seven Day Circle: The History and Meaning of the Week*. New York: Free Press or Collier Macmillan, London. 1985. Pp. xiv, 206. $16.95.

The thesis of Eviatar Zerubavel's book is quite clear: the seven-day week is a social convention, indeed, a reification. Unlike the day, the month, or the year, which are derived from the cosmic movement of the sun and the moon, the week is not a natural phenomenon. Rather, it is derived from the human imagination and has no concrete basis in nature. The origin of the week is found in religion, specifically, in the religious cosmogonies of the early Hebrews and the Chaldeans. As Zerubavel notes, in obvious reference to the biblical story of Creation, the week "was first practiced by God when creating the universe" (p. 6). The Jewish celebration of the seventh day as the sabbath is therefore a distinctively Jewish contribution to civilization. The Hebraic emphasis on the absolute transcendence of God led the early Hebrews to establish the week as independent of nature and the cosmos. This theological presupposition resulted in most people of the modern world ordering their lives around a seven-day cycle.

The Chaldeans developed a planetary theory of the seven-day week, which was apparently used primarily for astrological reasons. But the actual establishment of the seven-day week from this Chaldean perspective was not until after the conquests of Alexander the Great. Eventually, the Jewish and astrological weeks merged around the first century B.C., and the Roman names for the days of the week were adopted. The institution largely responsible for the spread of the week was the church: "It was the Church that was responsible for integrating the Jewish and astrological weeks together and spreading the seven-day cycle throughout most of the world" (p. 20). Despite futile attempts to change the week, such as in the French and Russian revolutions, the theological view of the week has prevailed.

After discussing other views of the week, ranging from the five-day to the twenty-day week, Zerubavel analyzes the sociological and practical consequences of the seven-day week on human life. He explains why Monday is often referred to as "blue Monday" and Friday dubbed "TGIF-day" on the basis of the rhythmic cycle of workdays and rest days. In other words, the week "functions not only as a standard unit for measuring the passage of time, but also as a framework within which activities can be organized in a systematic fashion" (p. 100).

Zerubavel's book is one of a kind, painstakingly researched, clearly written, and fascinatingly informative. It provides further documentation of the religious roots of an age that frequently thinks of itself as nonreligious. Historians of varying fields should read it, especially those interested in theories of secularization.

HARRY J. AUSMUS
Southern Connecticut State University

ROBERT J. ROSS and GERARD J. TELKAMP, editors. *Colonial Cities*. (Comparative Studies in Overseas History, number 5.) Boston: Martinus Nijhoff, for the Leiden University Press; distributed by Kluwer Academic, Hingham, Mass. 1985. Pp. 250.

Leiden, once the intellectual seat of Dutch colonialism, has regained its dominant place through the establishment of the Centre for the History of European Expansion. The volume under review is the fifth in the series of thematic collections devoted to special topics in the analysis of colonialism, covering political, economic, and even intellectual history. Indeed, this collection is interdisciplinary, bringing together geographers, sociologists, and historians.

Given the wide variety of connotations of the term "urbanism," the organizers of the conference carefully define their subject matter as "i. a town that was founded or developed under the influence of a Western colonial power; ii. having a central function in the colonial administrative or economic system; and iii. having a substantial portion of Western elements" (p. 232). This led most of the authors to focus on the largest city in their particular colony or, in the case of India, presidency, but it did not limit inquiry to a single set of questions.

Three of the essays deal with the general topic of colonial cities. After a brief introduction by the editors, Robert J. Ross and Gerard J. Telkamp, the tone is set for the collection by Anthony King, a scholar of encyclopedic knowledge whose writings have covered the full range of urbanism. His eleven-

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thousand-word essay, the longest in the collection, succinctly summarizes an extremely complicated topic. Taken with Josef W. Konvitz's new book on urbanism in the developed world (*The Urban Millenium* [1985]), this essay provides the beginner with easy access to a considerable body of literature. Returning to King's themes, the geographer G. A. de Bruijne discusses the implication of the colonial experience on postindependence cities.

The other essays constitute the mixed bag that one usually expects from workshop proceedings. All are short: J. L. Miège's describes Algiers under the French in barely four thousand words. Given these constraints, the best essays are those that treat a special topic over a relatively short period of time.

Indeed, some of the essays on seventeenth- and eighteenth-century cities are jewels. The late Adriaan van Oss, to whom the volume is dedicated, provides a stunning cartographic analysis of Central American urban development, demonstrating that the cities there served to enrich local colonists rather than the Spanish empire. Equally spectacular is Leonard Blussé's analysis of how sugar production degraded the environment of old Batavia (Jakarta), transforming it into a sinkhole of disease that the Dutch were forced to abandon. By contrast, P. J. Marshall argues that Calcutta, free of the short-sighted monopolism of the British East India Company in its last years, brought undeniable benefits to its residents and its Bengali hinterland. J. L. Oosterhoff shows that the Dutch settlement of Zeelandia on Taiwan might have produced similar prosperity had it not fallen victim to the new Ch'ing dynasty.

In the remaining essays, with the exception of Raymond Betts's analysis of Dakar as administrative capital of the French West African federation, the authors allow their analyses to be swamped by their data. The three essays on cities in which slaves constituted a substantial proportion of the population—Ross on Cape Town, Mary Karasch on Rio, and Colin Clarke on Kingston, Jamaica—all demonstrate familarity with the literature but fail somehow to cohere. Considerations of space also limit X. Guillaume's treatment of Saigon's changing administrative role and Dirk Kooiman's discussion of polarities in nearly three hundred years of Bombay's history.

Despite the brevity of these essays, the volume as a whole constitutes a real contribution to the study of colonial urbanization. Indeed, it revitalizes a field that seemed too diffuse for systematic analysis. Even after a four-year delay and despite some lapses in English translation, the Leiden group is pointing the way to new insights on the nature of colonialism.

BRUCE FETTER
University of Wisconsin, Milwaukee

NED C. LANDSMAN. *Scotland and Its First American Colony, 1683–1765*. Princeton: Princeton University Press. 1985. Pp. xiv, 360. $35.00.

This ethnic history seeks to examine the nature of Scottish society at home and in colonial New Jersey in order to understand the relationship of Scottish traditions, colonial environment, and ongoing transatlantic influence in defining the Scottish community in the New World.

Ned C. Landsman begins with a study of Scotland in the seventeenth and eighteenth centuries, when the country was still overwhelmingly agrarian. The patterns of landownership therefore determined a social structure fundamentally different from England's. The differences are most clearly revealed when the author combines statistical data with more traditional sources. Landsman links a variety of records to examine regional social structure and ascertain differences in landholding patterns, population stability, and geographical mobility. He notes that, when differences existed in religious affiliation, acculturation to English ways, and willingness to accept change, particularly in agricultural practices, they were accentuated by the interplay of perceptions of geography and geographic realities. In the later part of the seventeenth century, significant change was heralded by a general move to expand and improve the entire economic structure. In the process new forms of wealth and expanding economic opportunity emerged overseas. The Scots' colonization of East Jersey is seen, therefore, as an extension and continuation of a prevailing attitude at home in the seventeenth and eighteenth centuries.

In the latter part of his work Landsman carefully examines the extent to which Scottish conditions and traditions affected the development of East Jersey. By detailing and explaining the pattern of growth during the time of the Scottish proprietors, he adds to our knowledge of the way they shaped this middle colony so that New Jersey's settlement differed in many respects from neighboring colonies. How and why the Scots maintained and in many instances intensified their ethnic identity is perhaps the most interesting part of this work. For the Scots the interaction of ethnic influence and environment tended to disrupt and fragment the Scottish community. Combined with a tendency among many influential Scots to anglicization, this should have rapidly diminished Scottish identity. That it did not can be partly attributed to the nature of settlement in New Jersey. Although the population was heterogeneous, especially in East Jersey, the colony paradoxically produced not a mixture of peoples but ethnic enclaves. Such a system of population settlement tended to reinforce Scottish linkage with the Old World, thus reinforcing the Old

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World structure. At the same time, and often as a result of political or religious upheaval, it gave rise to change that in turn produced a new solidarity, a different ethnicity that the settlers adopted for their own. These influences then helped reinforce the sense of community that made Scottish settlement readily identifiable both as Scottish and eventually Scottish-American.

Landsman's work is an ambitious undertaking. If on occasion he tries to place too much information and too many concepts in a single short work, overall he succeeds admirably. His view of change encompasses both the Old World and the New, and he presents it as a dynamic, interactive, and ongoing process involving an amalgam of culture and environment on both sides of the Atlantic. By so doing, Landsman has made an important contribution of fact and methodology to our understanding of the colonial period.

JAMES H. LEVITT
State University College of New York, Potsdam

ANTHONY ARBLASTER. *The Rise and Decline of Western Liberalism.* New York: Basil Blackwell. 1984. Pp. xi, 394. $34.95.

Anthony Arblaster's critique of liberal ideology seeks to redress the allegedly dominant view of liberalism that ignores its "darker side"—its indifference to the plight of the poor and to the claims of social justice, its insistence on acquisitiveness as the ruling passion of human nature, its latent conservatism, and its retreat from its own revolutionary origins into privatism and complacency.

Arblaster's account adds little to the social democratic critique of liberalism already advanced by C. B. Macpherson and Steven Lukes and even earlier by R. H. Tawney. The distinguishing feature of this social democratic critique is the claim that individualism is the "key to the understanding of liberalism" (p. 53). Arblaster finds individualism embedded even in the philosophical foundations of liberalism, especially in the writings of Descartes, Hobbes, and Locke, which treat the world as a "chaos of individual entities" (p. 140) and thus postulate a kind of "ontological individualism" (p. 38).

The attempt to trace the philosophical premises of liberalism is to be commended. A more penetrating examination of those premises, however, indicates that individualism is not the main issue. The most important premise underlying the liberal tradition is the equation of knowledge with certainty—more specifically, with the positive knowledge supposedly achieved by modern science—and the corresponding devaluation of practical reason, of knowledge that is contingent, finite, and limited. The Promethean revolt against limits most clearly defines the liberal world view and explains why liberals tend to waver between social engineering and a debilitating political skepticism.

Once reason is identified with positive science, values must either be relegated to the realm of private choice or themselves be made the object of scientific inquiry. The first path, as Thomas Spragens explains in *The Irony of Liberal Reason* (1981)—a good corrective to Arblaster's account—leads to the conclusion that politics can have nothing to say about values; the second, to the conclusion that a new science of society will uncover laws of "behavior" and thereby replace politics with administrative expertise designed to encourage good behavior and discourage bad. This second line of development finds intellectual expression in the recent enthusiasm for behavior modification, genetic engineering, and artificial intelligence. It finds its most striking political expression in states professing one or another variety of "Marxism" as their official ideology.

Arblaster's preoccupation with individualism leaves the technocratic side of the liberal tradition completely unexamined. This one-sided view makes it impossible for him to understand the twentieth-century revulsion against utopianism except as a betrayal of liberalism's revolutionary past. Arblaster deplores this "withdrawal" but finds himself unable to explain it. Faced with a bewildering turn to the right, he can only fuss and fume, unaware of its origin in the development of liberalism itself. Only a perspective that transcends the conventional polarization of liberalism and "radicalism," or Social Democracy, will be able to make sense of these matters.

CHRISTOPHER LASCH
University of Rochester

RANDOLPH L. BRAHAM, editor. *Jewish Leadership during the Nazi Era: Patterns of Behavior in the Free World.* (Holocaust Studies Series, East European Monographs, number 175.) New York: Social Science Monographs or the Institute for Holocaust Studies of the City University of New York; distributed by Columbia University Press, New York. 1985. Pp. xiv, 154. $20.00.

The five essays in this book are an outgrowth of a conference held in November 1981, the purpose of which, Randolph L. Braham writes, was to provide "an in-depth, objective, and systematic investigation of the attitudes and reactions of the Jewish leaders of the free world during the Nazi era" (p. viii). Although it is impossible to be objective about the Holocaust or anything connected with it, the essays otherwise accomplish their tasks and thus contribute

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to the study of this period in our history. They also indict Jewish leadership, especially in the United States and Palestine. There are few apologies here, although it would be easy to make them, and little effort to sweep the failings of Jewish leadership during this period under the rug.

All of the essays consider the problems that Jewish leadership faced when dealing with indifferent or hostile governments. The first four, in particular, demonstrate the inability of world Jewry either to influence governments or to organize and focus its efforts. Bela Vago shows that the *Yishuv* (the Jewish community in Palestine) encountered unyielding British resistance to Jewish immigration, and Bernard Wasserstein points to the reasons for this intransigence: British anti-Semitism and British concern with Arab opinion. Wasserstein also shows the lack of political clout of British Jewry and its consequent inability to induce the government to assist Jews on the Continent. Yet, although Jewish leadership in Britain failed in its efforts, it "was not for want of trying" (p. 43). American Jewish leadership, as David S. Wyman shows, also faced a government that was often indifferent and at times hostile. But, unlike the leadership among Britain's Jews, American Jewish leadership contributed to its own ineffectiveness. Diffidence, internal squabbling, and an unwillingness to believe confirmed reports from Europe impaired its ability to effect some change in policy on the part of the American government. Gerhart M. Riegner is ambivalent about Jewish leadership in Switzerland. Yet he reminds us that the tiny Swiss Jewish community had to deal with a government influenced by "a deep sense of anti-Semitic prejudice . . . among a considerable part of the population" (p. 72), within a nation surrounded (after 1940) by Axis forces. And the Jewish community did persuade that government to take in a limited number of Jewish refugees, especially after the government in Fascist Italy collapsed.

Throughout most of this period (1933–45), Jewish leadership not only was unorganized but also could not often agree on a purpose. This was especially true in the United States and Palestine. Thus, Jewish leadership was in a collective shambles precisely in those areas where a collective effort might have been efficacious. The basic confrontation was between those who felt that the establishment of a Jewish homeland demanded their attention before all else and those who wanted to concentrate on rescue efforts. After the Bermuda Conference (April 1943), which demonstrated the lack of concern for European Jewry on the part of the United States and Britain, American Jewish leadership moved increasingly toward the goal of Zionism. The *Yishuv* leadership was always concerned with moving Jews into Palestine, even "at the expense of the attention that the fate of European Jews should have commanded" (p. 65). Only late in the war did they try to get the Allies to blow up the rails to the death camps.

The last essay, which was added "to fill a gap discerned during the . . . conference" (p. ix), deals with Latin American Jewry and is a good, brief history of Jewish organizations, communal structures, and leadership south of the United States. This is less an examination of Jewish leadership during the Nazi era than a general history, and Haim Avni thus gives limited attention to the central focus of the book, although he does cite efforts among Latin American Jews to protest events in Germany and to appeal to their governments to aid in Jewish immigration, in which they were moderately successful.

These essays point out that in all too many ways Jewish leadership in the period from 1933 to 1945 failed. Yet, Jewish guilt and self-recriminations aside, for all of its errors Jewish leadership did not build the death camps or light the ovens.

ALAN EDELSTEIN
Towson State University

CHRISTOPHER THORNE. *The Issue of War: States, Societies, and the Far Eastern Conflict of 1941–1945*. New York: Oxford University Press. 1985. Pp. xix, 364. $22.95.

This book is a worthy sequel to Christopher Thorne's earlier study, *Allies of a Kind* (1978). Thorne's general objective is to put what Americans are wont to call the Pacific war, which he calls the Far Eastern war, into a broader setting. This, he argues, corrects the hitherto-narrow focus of thinking about the war, emphasizing its worldwide nature, with special attention to the British, French, Dutch, Indian, Southeast Asian, and Australian aspects. In this framework Gandhi receives more attention than Tojo, and the Netherlands' archives are well used but the Japanese archives not at all.

If this seems rather strange or forced, it should be said that the author gives both long- and short-range justifications. The long-range consideration is that, when viewed in its broader ramifications, this Far Eastern war greatly speeded up the processes of history, both in terms of "social time" and "geographical time" (p. 320). Thus, it "created an entirely new pattern of Southeast Asian politics" (p. 153), with the "humiliation of the white man" there and in India (pp. 162, 168). It taught "international society" that "the nation-state must in some way be transcended or even superseded as the basic operating unit and focus of loyalty" (p. 313), and the United States, "winner of the Far Eastern war in 1945, could by the 1970s be seen as having been in

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certain senses the greatest of its long term losers" (p. 325).

This is dramatic stuff, and "dramatic" is a good way to describe Thorne's approach, as is indicated in his chapter titles. After discussing "the approach of war" in its international and domestic contexts, the major portion of the book takes up the experience of war under the following chapter titles: "Facing the Enemy," "Winning Friends and the Future: Japan and Asia," "Winning Friends and the Future: The West and Asia," "Facing One's Friends," "Life, Death, and Change," and, finally, "Facing Oneself and the Future." Perhaps Thorne's conceptualization is too futuristic and philosophical, but it should also be said that Thorne presents a wealth of historical detail to back up his wide-ranging approach to the big questions.

Also, the author candidly admits that his approach "has been shaped in part" by his inability to read Japanese or Chinese records as well as "the quantity and quality of historical material available to me in other respects" (p. xi). He compensates for this by making good use of monographs and collections by scholars who have used these—such as the works of Akira Iriye, Robert J. C. Butow, James Morley, Lloyd Eastman, Sadako Ogata, Richard Storry, and John H. Boyle—although it hurt my feelings a bit to note that he missed my (admittedly hard to find) book, *China and Japan: Search for Balance* (with A. D. Coox) (1978), and my article, "The Strange Diplomacy of Admiral Nomura" (*Proceedings of the American Philosophical Society* 114/3 [June 1970]).

Nevertheless, I strongly recommend the book, for Thorne's coverage and use of British, French, Australian, Dutch, New Zealand, and Indian documentation adds new dimensions to standard studies of the era and the war, whatever one calls it. And, although his insistence on using the term "Far Eastern" may be a bit overdone (it is, after all, Eurocentric), the long-range questions and (some of the) answers it produces are stimulating, exciting, and challenging.

HILARY CONROY
University of Pennsylvania

ANCIENT

ROBIN OSBORNE. Demos: *The Discovery of Classical Attika*. (Cambridge Classical Series.) New York: Cambridge University Press. 1985. Pp. xiv, 284. $49.50.

Almost thirty years ago Louis Gernet made the characteristically simple but compelling observation that all the inhabitants of Attika were called Athenians. Territorial and political unity around a civic center was fundamental, and modern notions of a town and country split are essentially misconceived for a polity where landholding was synonymous with citizenship. Robin Osborne's book tries to examine this unity and to create a fuller sense of how the village demes and their citizens interacted with one another and with Athens than earlier work with a prevailingly constitutional approach has permitted. His focus is social, and he commands an enviable range of literary, epigraphical, topographical, and archaeological data informed by newer approaches in comparative anthropology, sociology, and religion. Although the book begins with a detailed analysis of a lively legal case to suggest the participants' social networks, Osborne prefers to proceed at the level of general theory.

The work falls into two parts, a central thesis followed by excurses to support and qualify it on quarrying, silver mining, kinship patterns, and religious practices. Osborne portrays the demes as necessarily close, active communities. A nucleated settlement pattern in which families lived in the deme villages and not on isolated farms fostered a strong sense of solidarity, as did the emotional ties of families to the deme land and membership they inherited. Tying the localities to the civic center was the demes' critical political role in registering males as citizens at eighteen and in selecting men annually for the Council of 500. Without a deme a man had no political identity; the maintenance of local connections, with honors received for benefactions given, provided aspirants to political careers in Athens with justification for and help in obtaining their goal.

This is an attractive view, but it is not completely convincing. The problems are twofold: the nature of the evidence and the uses to which it is occasionally put. None of our data provide genuinely random samples from which statistically valid generalizations can be drawn. Skewed in a number of ways, the evidence is overwhelmingly focused on families of wealth and power, containing practically nothing about the majority of the men in the localities ostensibly being considered here and worked into a general theory. The presentation of data in convenient tabular form, with discussions of distributions and percentages, and the detail of the supporting chapters (much of it unnecessary) conceal the depressingly scrappy and disjointed information available for this undertaking.

Any argument for strong deme ties must confront the fact that a citizen's membership was inherited and had no necessary connection with birthplace, place of residence, or location of significant property holdings. Tracing social relations through individuals' memberships in demes thus risks being a seriously misleading methodology. For the elite,

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about which we know most, landholdings and other economic activity were scattered, as were marriage ties. Even supposing a substantial holding in the deme of registry was prevalent (the evidence presented here does not support this), it is not self-evident how this entails the kind of local identification or cohesion among the rich or less rich implied in Osborne's theory of deme solidarity. At times he takes cases that appear more exceptional than not (Philokrates or tombs in Rhamnous, for example) only to turn them subtly into paradigms. And statements such as one suggesting that the insistence on using local stone in deme building manifests local solidarity and not, as Osborne himself has shown, difficulties of transport suggest an author too in thrall to a theory.

Despite these caveats and criticisms, this remains a worthwhile and obviously provocative study. The evidence it gathers and the approach it essays should encourage and enable others to pursue the issues it raises. We still are a long way from understanding how deeply, for instance, local festivals and dramatic presentations in deme theaters cultivated deme loyalties while forming linkages through imitation with the civic center. And we can still wonder about the implications of the discontent that Thucydides so memorably described when rural dwellers had to move to the city as a result of Perikles' policy. It is to Osborne's credit to have started asking the right questions.

W. E. HIGGINS
Jersey City, New Jersey

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MARTIN FREDERIKSEN. *Campania*. Edited by NICHOLAS PURCELL. London: British School at Rome. 1984. Pp. xviii, 368. £17.00.

Martin Frederiksen, who labored at the history of Campania over the course of decades, left at his death this book, which is incomplete both in conception and execution. It is too late to question whether the author would have chosen to publish what we have before us. This is not a total history of ancient Campania, for Frederiksen left many gaps in his presentation. Furthermore, some of the chapters were composed long ago (including reworked publications of the late 1950s and a chapter completed in 1968) but have been equipped with a more up-to-date bibliography and footnotes that cannot conform to the text they purport to serve. Other chapters are previously published pieces that Nicholas Purcell has also reworked and, in one case, entirely recast in our mother tongue. Finally, the present work is badly edited.

Most of the fourteen chapters are thin because they supply learned debate more than general narrative and because the discussion is often outdated. Other chapters are thin because they never were completed by the author. Generally speaking, the history of Campania and all of its component towns after the fourth century B.C. is virtually absent from the book. A reader who seeks knowledge on large matters but who has little control of ancient history will be at sea when he reads this book. Experienced ancient historians will be frustrated by what is incompletely treated or not treated and wearied by idiosyncratically abbreviated references and citations (besides the many references to page zero). Two plates (1 and 2) are useless; seven plates with inscriptions (7 to 13) have incomplete identifications; and the captions are equally useless for pursuit through the index, which is less complete, if possible, than the text.

Given the circumstances of this book's publication, it is not surprising that the text contains inconsistent statements, even of fact. At the outset Frederiksen makes much of the geopolitical unity of Campania. But the resulting patchwork disappoints, and, other than in the case of the land itself, he writes nothing on the geopolitical unity of the region purported to be peninsular Italy's greatest breadbasket in antiquity.

ROBERT E. A. PALMER
University of Pennsylvania

BRUCE W. FRIER. *The Rise of the Roman Jurists: Studies in Cicero's* pro Caecina. Princeton: Princeton University Press. 1985. Pp. xxiv, 317. $36.00.

This book takes the form of a discussion of Cicero's speech for Caecina, which is his only speech delivered in a private law case surviving in complete form as published and one that Cicero himself seems to have thought highly of. Bruce W. Frier considers the case (ultimately concerning ownership of a farm) from the points of view of the different participants in the judicial process; his account is thorough and variously informative, if at times, as he recognizes, speculative. Here I shall only note the general issues for the history of Roman, and thus all of Western, law that Frier sees arising from it.

For Frier, the late second and early first centuries B.C. at Rome were a period of great legal insecurity, owing both to the recent rise to dominance of the rhetorically trained advocate in the courts and to the number of broad procedural reforms introduced by praetors in their edicts, which varied from year to year. Judges were laymen by tradition, not persons skilled in law. During this period, however, great nobles (with many other preoccupations) began to give opinions (*responsa*) that advocates might refer to in court (to judge by the *pro Caecina* often without naming their authors). But, apparently, orators frequently argued that the court need not take account

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of such opinions or even of the *ius civile* itself. There was no agreed way of determining what the law really meant; thus, modern scholars who claim that Cicero was "wrong" in his interpretation of an interdict in the *pro Caecina* have misunderstood the position.

In the last decades of the Republic, Q. Mucius Scaevola's writings for the first time imposed some organization on the civil law and introduced much greater breadth and rigor into its analysis. No previous society had paid such close attention to a body of secular law. The nobles concentrated on advocacy in order to acquire political support; men of slightly lower status devoted themselves to the new legal science as true professionals. The *pro Caecina* presages these changes; here Cicero argues that juristic *responsa* should not be disregarded and eulogizes a largely "autonomous" law. The first emperors gave special authority to chosen jurisconsults, and cases were more frequently decided on their advice. Frier argues that social causes precipitated the change: concurrent with the growth of wealth and commerce was the expansion of the citizen body, which increased the workload of the courts. Political instability created a desire for stability of some kind in personal and property rights and necessitated the establishment of cut-and-dried rules.

Some aspects of this picture are uncontroversial; others are new, interesting, and often persuasive. One should, however, set the new developments in an even broader perspective; the drive toward organization and more sophisticated argumentation is part of the dramatic intellectual advance made in every field at this period, and to some extent it was inevitable (on a point of detail, the *Lex Cornelia* limiting the praetor's discretion must be related to the contemporary campaign against senatorial corruption). And, although Frier is aware of the limits of our evidence, he sometimes presses it hard. The role of Q. Mucius is perhaps overdramatized, and we cannot be sure that Cicero was really saying something new about law and lawyers in the *pro Caecina*. And might he not have said the opposite the next day in another case? Also, the relevance of a rise in commerce is probably exaggerated.

But this is a distinguished work. The author's knowledge of recent literature on Roman law and history and on general legal questions is impressive, and although the arguments with which he is dealing are sometimes specialized and often dense, he writes with almost unfailing elegance. Roman historians, as well as lawyers and those interested in the history of law, ought all to read and learn from the book.

ELIZABETH RAWSON
Corpus Christi College
University of Oxford

STEPHEN BENKO. *Pagan Rome and the Early Christians*. Bloomington: Indiana University Press. 1984. Pp. xi, 180. $20.00.

Stephen Benko wishes to examine how pagans looked at Christians and why pagans reacted the way they did. In this book early Christianity essentially means the second century, although frequent reference is made to later authors and events. As for paganism in Rome, Benko treats it as synonymous with paganism in the Roman Empire; the definition of pagan (p. 25 n. 9) should, however, have been formulated to exclude Jews as well as Christians. The author has already published "Pagan Criticism during the First Two Centuries, A.D." (*Aufstieg und Niedergang des römischen Welt* 23/2 [1980]: 1055–1118) but now addresses a larger public. His book is informative and well documented, even if it avoids technical discussions and may not be as coherent as expected. Because of the scarcity of sources, some chapters offer more about the Christian than about the pagan point of view.

Chapter 1 deals with the persecutions of Christians. When they complained that they were persecuted only because of their name, Christians must have been aware that their name aroused basic fears in pagans—fears about the overthrow of the gods, the disconcertment of exclusive claims, and the upset of the prevailing social order. They also knew that their name evoked images of wrongdoing of different kinds. Before considering this last point, Benko offers, in "The Portrait of an Early Christian," Peregrinus, whom we know from Lucian of Samosata. The title of the chapter is ambiguous, since Peregrinus, who lived as a Cynic, may have renounced altogether the Christian faith after his excommunication from the church. Benko suggests that a certain empathy and association existed between some Christians and some Cynics and that this connection was not helpful to Christians as a whole.

Chapter 3 discusses alleged obscene practices and cannibalism among Christians. Benko feels that libertine trends had to be fought very early in the church and that pagans may have failed to distinguish between mainstream Christianity and Gnostic sects like the Phibionites (see his study, "The Libertine Gnostic Sect of the Phibionites according to Epiphanius," in *Vigiliae christianae* 21 [1967]: 103–19). Chapter 4, on the holy kiss, could be considered as a postscript of the preceding chapter. The rite posed problems for Christians and may have contributed to the rise of infamous rumors, but nothing directly supports the hypothesis (on page 83, Benko states that Augustine wrote that the holy kiss implied a contact of the lips; on page 24 he refers only to N. J. Perella's *Kiss Sacred and Profane* [1969], which correctly quotes from Augustine's

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sermon 237). Chapter 5, on magic, is particularly original and interesting. If there is no evidence of the involvement of Christians in harmful or malevolent magical arts, they did perform exorcisms and miracles, speak in tongues, and use objects and words charged with divine potency. It is therefore not surprising that pagans associated them with magic. Chapter 6, on pagan criticism of Christian theology, briefly examines the positions of Galen and Celsus.

The one-page summary recalls that pagan adversaries of Christianity were not "gullible fools" and that "they hit on sensible nerves" in different circumstances. The statement about charges of immoral behavior, however, seems to go beyond the conclusions reached in chapter 3. There is a good bibliography. In a word, we are presented with a readable, well-balanced, and useful book.

EMILIEN LAMIRANDE
University of Ottawa

STEPHEN WILLIAMS. *Diocletian and the Roman Recovery*. New York: Methuen. 1985. Pp. 264. $19.95.

Diocletian was one of the truly important emperors and certainly the most original. Yet until now we have had no biography except for W. Seston's *Diocletien et la tetrarchie* (1946), of which only the first volume was ever published. The poverty of the narrative sources is the obvious reason for this neglect. Without a Suetonius, Tacitus, or Ammianus to provide flesh for the archaeological, epigraphic, and numismatic bones, the biographer's task is undoubtedly hard.

That common impediment to late imperial biography, however, is much less serious in the case of Diocletian because the sheer magnitude and originality of his achievement could not but generate a great deal of nonnarrative evidence. Some of this evidence is of large specific value and certain attribution, such as the epigraphic Edict on Prices of 301, which reveals not only a policy but also a mentality; some is undated but can nevertheless be convincingly attributed to Diocletian, such as the Notitia Dignitatum's data on army organization chronologically dissected in D. Van Berchem's celebrated *L'armée de Diocletien et la réforme constantinienne* (1952); and some is internally dated but fragmentary, such as the numismatic advertisements of official policy, which, in Diocletian's case, are useful for defining the stages of his peculiarly "structural" politics.

As a result of his good use of the data in all three categories, Stephen Williams has been able to reconstruct a continuous "life and times" without any major speculative leaps between attested facts. The author begins with a brief descriptive chapter on the half-century of invasions, usurpations, and secessions of the third century, when Diocletian (then still Diocles) was born "probably in 243, probably at Salona" (p. 22). Next, Williams describes Diocletian's military career under Gallienus and his energetic Illyrian successors, men who rose through the ranks in the fighting on the frontiers, far from Rome. When Diocletian defeated his fellow Illyrian Carinus in 285 to make himself emperor, he was almost an old man by the standards of the time, and his twenty-seven years that followed defied all actuarial expectation.

Diocletian's originality surfaced from the start. The emperor paid his respects from afar but otherwise ignored Rome and its symbolic processes of legitimization; he did not even visit the city until the eve of his abdication, twenty years later. Instead, he attended to the restoration of imperial power, everywhere precarious. To have gone west and restored Gaul would have meant exposing the East, but to have sent another would have risked usurpation. Diocletian confronted the problem by following an ancient precedent in novel form: within months of his own elevation as Augustus, he appointed his fellow Illyrian Maximian as Caesar and Filius Augusti in charge of the West.

In the chapter on the tetrarchy, Williams is at his best in explaining that the rule-of-four was actually a retroactive systematization of Diocletian's pragmatic improvisation. In 293 Maximian, now an Augustus himself, appointed his own Caesar (Constantius) and Diocletian did likewise (Galerius); both were to abdicate one day in favor of the two younger men, who then appointed their own Caesars. In the next two chapters, Williams surveys the restoration of imperial control. With four vigilant rulers guarding one another's power, reconquest accelerated. The frontiers were not merely restored but even consolidated, leaving out the less defensible parts to allow more adequate defense of the most valuable areas. The author then describes Diocletian's new system of imperial defense: a strongly fortified frontier manned by resident troops, with counterattack cavalry forces held in the immediate rear within each sector and with legionary fortress troops to backstop the system in the deep rear.

In the last section the new empire created by Diocletian is described and explained in successive functional chapters on the governmental structure, taxation and finance, economic regulation, provincial administration, religious policy, and the Christian problem. Finally, Williams reverts to chronological narrative to describe Diocletian's totally unprecedented voluntary abdication (along with the reluctant Maximian), the breakdown in the tetrarchic system that ensued, and the reversion to a central mobile army, which unitary rule made inev-

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itable and which geography made of disastrous effect.

It is symptomatic of the broad appeal of Roman historiography that the author of this valuable book is not an academic classicist. He has nevertheless performed a valuable service for the profession as well as for the general reader. Williams produces no new data of his own, but his judgment of particular matters is invariably sound, and the book is a well-written synthesis.

EDWARD N. LUTTWAK
Chevy Chase, Maryland

JOYCE E. SALISBURY. *Iberian Popular Religion, 600 B.C. to 700 A.D.: Celts, Romans, and Visigoths.* (Texts and Studies in Religion, number 20.) Lewiston, N.Y.: Edwin Mellen. 1985. Pp. 334. $34.95.

This book deals with some one hundred years around A.D. 600 in parts of Galicia. Joyce E. Salisbury examined the thorough historical studies of A. K. Ziegler and his students at the Catholic University of America, including unpublished master's theses, and found them wanting. In her effort to explore the culture of the Galician village she therefore introduces anthropological methods and archaeological evidence as correctives to the literary historical sources. She freely mixes sources and secondary works, such as Carlo Ginzburg's *The Cheese and the Worms*, but the secondary works determine her outlook.

In Salisbury's view, society consisted only of rulers and peasants, the clergy being allied with rulers in pursuit of power and uniformity. Rural priests sometimes mediated between distant bishops and the people, who generally satisfied their religious needs with the remnants of Celtic pagan ways. Monasteries were of less importance than holy hermits and fertility virgins, who were not part of the ecclesiastical system but whose social importance derived from their influence over the forces of nature. The blood of the martyr Eulalia is identified with the menstrual blood of the pagan virgin (pp. 119–20); this forty-five-page chapter on holy hermits and virgins as mediators is the longest of eight in the book. Priscillianism was a reaction against ecclesiastical control of religious life. Although works written by ecclesiastics are scanty, they are our principal source of information about rural religion.

If this is Salisbury's definitive word on religion in Galicia, she may transfer her next efforts to Anglo-Saxon England in Pope Gregory's time or to some other area where early medieval missionaries worked.

EDWARD COLBERT
Mattoon, Illinois

MEDIEVAL

MICHAEL LAPIDGE and DAVID DUMVILLE, editors. *Gildas: New Approaches.* (Studies in Celtic History, number 5.) Dover, N.H.: Boydell and Brewer. 1984. Pp. xii, 244. $37.50.

Gildas the Wise, more than most medieval authors, has been denounced for not writing with modern scholars in mind. As the only extant British source for the late fifth and early sixth centuries, he is primarily a homilist inveighing against the sins of his fellow countrymen, but modern historians want hard data on Arthur (whom Gildas does not mention), Mount Badon (which he does), and other "dark" matters. Recent theories of E. A. Thompson and Michael Winterbottom (to whom this volume is dedicated), T. D. O'Sullivan, Molly Miller, and others have, with new questions, excited and often irritated others into response. *Gildas: New Approaches*, concentrating on Gildas's text itself, pours new wine into a newly shaped bottle.

The first two essays emphasize historical continuity, political and cultural. Ian Wood analyzes both insular and Continental sources, which *in fine* cast doubt on cataclysmic theories of transition in sub-Roman Britain and indicate continuing Roman political influence a generation after A.D. 410. This continuity is supported by Michael Lapidge's survey of the cultural context of Gildas's education; Gildas's style, less idiosyncratic and "Hisperic" than often assumed, indicates nonmonastic, traditional Roman rhetorical education and facility in a living, even Ciceronian, Latin. Close stylistic and textual analysis continues in four new approaches. Neil Wright stresses Gildas's innovations of word order complexities and poetry-influenced prose but relates these to knowledge of classical literature and, again, a more elegant style than is often admitted. Giovanni Orlandi analyzes, with statistical tests, Gildas's use of *clausulae*, the concluding sequence of syllables; Paul Schaffner studies Gildas's Bible-influenced use of the word *iudices* as a general term for "rulers." In a literary approach to Gildas's imagery, A. C. Sutherland demonstrates the combination of Christian and Roman, spiritual and physical elements that heighten Gildas's moral judgments on the sins of the British.

Three essays center on problems of chronology and geography. David Dumville's "Gildas and Maelgwn" concludes that neither the *Annales Cambriae* nor Welsh genealogies confirm traditional identities or dating of the five British kings attacked by Gildas and by whom scholars have tried to date his writing. In Dumville's subtle study of the chronology of book 1 of *De Excidio*, complex uncertainties predominate but with strong arguments for northwestern England as the locus and 525–50 A.D.

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as the time of Gildas's writing. Wright's case for all Britain as the locus of Gildas's history somewhat modifies this in his study of the wars against the Picts and English.

Patrick Sims-Williams's speculations on the relation of Gildas and vernacular poetry illuminate the role of early bards, the relationship of Celtic and Latin culture, and "dragon" and "lion" imagery in Celtic praise-poetry. The volume concludes with Richard Sharpe's essay on Gildas's influence on canon law and Dumville's essay on the identification of Uinniau, Gildas's correspondent. The editors hope the volume will "help set Gildasian studies on a number of new and potentially profitable paths" (p. x), which these twelve seminal essays certainly do. As every historian knows, the answers one gets depend on the questions one asks, and Gildas is at last being asked the right questions.

WILLIAM A. CHANEY
Lawrence University

BERNARD MCGINN. *The Calabrian Abbot: Joachim of Fiore in the History of Western Thought*. New York: Macmillan. 1985. Pp. xix, 261. $24.95.

Joachim of Fiore (1135–1202) is one of the most difficult and intriguing thinkers of the Middle Ages. In recent decades the work of Morton Bloomfield, Herbert Grundmann, Henri Mottu, Beatrice Hirsch-Reich, Majorie Reeves, and others has made Joachim more accessible to medievalists and to those interested in the history of Western apocalypticism. Now Bernard McGinn, another prominent Joachim scholar, proposes to confront the larger question of the Calabrian abbot's significance in the history of Western thought. As McGinn himself informs us, this is not a study made "from scratch"; it is a compilation of his own published and unpublished studies of Joachim and those of other scholars.

The first section of the book, "Background to Joachim's Thought," traces two parallel developments: the early Christian theologies of history, culminating in Eusebius's creation of the first universal theology of history in the fourth century, and the Tyconian-Augustinian exegesis of the Apocalypse, which was moral and antichiliastic rather than eventual and historical. The Tyconian-Augustinian mode dominated until Joachim, whose great contribution, according to McGinn (and others), was to reinstate the Apocalypse as the central prophecy of God's "plan for history." This treatment begs two important questions: was Joachim consciously reviving the submerged "Eusebian" theology of universal history and, if so, why? Seemingly, he was not alone in this: certain works of Rupert of Deutz, Honorius of Autun, and Anselm of Havelberg all point to the emergence of a new kind of eschatological consciousness in the twelfth century. McGinn touches on these questions but does not elaborate.

The second part, "The Main Themes of Joachim's Thought," centers on Joachim's famous division of history into three *status*. It is the last of these, the *status* of the Holy Spirit, to be ushered in by the *ordo monachorum*—preachers, contemplatives, and the Angelic Pope—that has provoked so much controversy. Did it constitute a vision of history that was no longer Christocentric? (McGinn says yes and no.) Who were these preachers and contemplatives who were to usher in the third *status*, and when would they appear? The latter question in particular is crucial to understanding Joachim's historical significance, and here the work of Reeves, John Leddy Phelan, and Alain Milhou is central, although McGinn does not draw them into his discussion.

Joachim's method of exegesis invites comparison with scholastic modes of discourse and commentary, and this is the subject of the final section of McGinn's book, "In the Wake of Joachim." Rightly leery of comparing scholasticism and Joachimism as such, McGinn compares Joachim with Thomas Aquinas and Bonaventure. The results are quite different. Thomas rejected Joachim in no uncertain terms, seeing him as fundamentally unscholastic in method and intention. Bonaventure, as Joseph Ratzinger has shown (and McGinn does not substantially differ), was in a more equivocal position. When he became minister general of the Franciscan order in 1257, he had to combat radical Joachimism within the order on the one hand and preserve the rich Franciscan heritage of Joachim's thought on the other. Only gradually did he incorporate certain elements of Joachim's apocalypticism into his own theology of history.

This final section, focusing as it does on these two admittedly important comparisons, leaves the question of Joachim's historical influence largely untouched. But in McGinn's defense it must be said that this is a question that cannot currently be adequately answered. The strength of this book is that it shows us what has already been accomplished and what tasks remain before us.

PAULINE MOFFITT WATTS
Sarah Lawrence College

KATHRYN L. REYERSON. *Business, Banking, and Finance in Medieval Montpellier*. (Studies and Texts, number 75.) Toronto: Pontifical Institute of Mediaeval Studies. 1985. Pp. xv, 184. $19.00.

This book represents only part of Kathryn L. Reyerson's extensive research on the economy of medieval Montpellier, and ideally it should be read in conjunction with her articles on that city's population, real estate market, silk industry, and wool

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and spice trades. With a population of almost forty thousand, Montpellier was one of the largest cities in early fourteenth-century France and, after the decline of Marseilles, became the kingdom's most important commercial link to the Mediterranean. Reyerson examines more than forty-six hundred business contracts recorded in thirteen extant notarial registers between 1293 and 1348 in order to study local business practices: types of partnerships and investments, the role of commercial and consumer credit, deposit banking, and foreign exchange. The book is less a history of business, banking, and finance than an investigation of the instruments and techniques of those activities in Montpellier. Reyerson succeeds in placing local practices within the context of contemporary business practices elsewhere, particularly in southern France and Italy.

In their business affairs the French failed to adopt all of the advanced techniques pioneered by the Italians. They continued to prefer small associations of family and friends to the large merchant companies with multiple branches favored by Italian businessmen, and they employed relatively few types of commercial instruments for a wide variety of purposes. The *comanda* (single investor) and *societas* (shared investment) contracts, for example, which the Italians generally reserved for maritime and landed transactions respectively, were adapted at Montpellier to either venture, depending on the needs of the partners. The most important contribution of this study is the illustration that credit was an essential ingredient of economic activity in all sectors and at every stage of production and distribution; virtually all business transactions contained some form of credit, though often disguised.

From her analysis of the participants in various types of contracts, Reyerson finds that two mutually exclusive economic systems operated at Montpellier—an urban-international and a rural-agricultural—each with a distinctive set of players, economic practices, approaches to investment, and responses to crisis. Well-to-do merchants, wholesalers, and money changers who possessed substantial liquid assets tended to invest in the risky but lucrative international trade, whereas those with fewer resources were limited to local investments, especially in land and houses but also in commodity futures. One wonders, however, whether the urban-rural dichotomy might not be overdrawn here as a result of the author's reliance on notarial registers (very few nobles and ecclesiastics resorted to notaries); a comparable analysis of Montpellier's hinterland might dispel the impression of an independent "subsistence-oriented" rural economy. In this respect it is regrettable that the book omits a discussion of the notarial registers themselves and the role of the notaries in Montpellier.

Although limited in scope, this book demonstrates the need for similar case studies of the urban economies of medieval France, whose countryside and rural society have preoccupied historians of late and where the linkage between town and countryside has never been adequately investigated.

THEODORE EVERGATES
Western Maryland College

SUSAN M. BABBITT. *Oresme's* Livre de Politiques *and the France of Charles V*. (Transactions of the American Philosophical Society, number 75, part 1.) Philadelphia: The Society. 1985. Pp. 158. $15.00.

Nicole Oresme (ca. 1323–82) inserted fifteen glosses at various points in his translation of Aristotle's *Politics*. He also expanded the text in other ways, usefully inventoried by Susan M. Babbitt (pp. 11–13), but his occasional glosses provide the matter for the present study. His king, disciple, and patron, Charles V, commissioned Oresme to translate the *Politics* as part of a wider effort to render some ancient and patristic texts on government into French. Quite reasonably, Babbitt maintains that Oresme's dominant purpose was not to recover Aristotle's interest so much as to adapt his doctrines to the circumstances of Valois France. Thus, for example, a number of his excursus treat of church politics and of the relation of royal to ecclesiastical jurisdiction.

Babbitt's method has been to gather statements by Oresme, scattered through his excursus, on particular subjects and to reassemble them under four headings: "National Sovereignty and the Hierarchy of Communities," "The Public State and the Common Good," "The *Politics*, the *Livre des politiques*, and the Church," and "Aristotle, Oresme, and Gallicanism." A chapter is devoted to each of these topics, and each chapter is divided into three sections: a description of the history or historical contingencies of the topic, a brief account of positions taken by earlier Aristotelian commentators regarding it, and, finally, the author's composite representation of Oresme's position. Regard for the context in which Oresme formed his doctrines is also evident in the two introductory chapters, which concern Oresme's career and the tradition of commentaries on the *Politics* that grew up and flourished between the completion of William of Moerbeke's Latin translation (ca. 1270) and Oresme's French rendering (ca. 1370–74).

The following sentence expresses Babbitt's appraisal of Oresme's political thought: "Oresme did not build up his political principles from the ground. First principles he left to others. . . . He accepted configurations of political power as he saw them, and did not inquire deeply into their origins" (p. 112). Thus, the quality of Oresme's judgments

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on the four topics selected for discussion varies. On the one hand, Oresme emphatically asserted the common good over the private interest of the prince, the jurisdictional autonomy of the church, and, in the midst of his demand for church reform, the accountability of popes for the common good of the community of believers. On the other, he was inconsistent and imprecise in his discussions of sovereignty; he largely ignored the tangled matter of relations between the royal and the episcopal offices; and he paid little attention to the origins of governmental authority, metaphysical or other. Clearly it was not his purpose, in his translation or in the interpolated glosses, to present a synthesis, a critique, or a systematic exposition of fundamental political doctrines. Babbitt's achievement is to have established the casual and, apparently, improvised character of *obiter dicta* that Oresme permitted himself in the context of his translation.

Any student of ideas must recognize a hermeneutic gap in this study, for it gives no indication of how Oresme posed and developed questions, working within or struggling against the limits of his genre and text. Babbitt's *Fragestellung* is clear and craftsmanlike; Oresme's is conspicuously absent. A complete appraisal of Oresme's thinking would surely have included, apart from assessment of the constraint of genre, a review of the great body of the translation itself, and not only of the incidental excursus. What is the relationship of the parts to the whole? The author states that, finding his native tongue inadequate to his purpose, Oresme "enriched the vocabulary of the French language by hundreds of words, even at the lowest estimate" (p. 10), which naturally implies syntactical modifications as well as neologisms. Was the translation free or literal? In either case, it was in itself an inventive commentary. Further, some topics of Oresme's glosses—Old Testament kingship, the active and the contemplative lives, geography, and natural slavery—are not discussed (so far as I can tell), nor, indeed, are Oresme's interpretations (in the language of the translation and in glosses) of Aristotle's key definitions, including those of the natural relations that constituted the household, the distinctions between political and personal virtue, the basis of community in friendship, and the goal of political union, not only as the sufficient life but also as a happy and honorable one. Aristotle's maxim that "political society exists for the sake of noble actions, and not of mere companionship," must have struck resonances in Valois France.

In asserting the obvious value of Babbitt's achievement, one must state that she chose, perhaps wisely, to portray Babbitt's understanding of Oresme rather than Oresme's understanding of Aristotle.

KARL F. MORRISON
University of Kansas

ROBERT I. BURNS, editor. *The Worlds of Alfonso the Learned and James the Conqueror: Intellect and Force in the Middle Ages*. Princeton: Princeton University Press, under the auspices of the Center for Medieval and Renaissance Studies, University of California, Los Angeles. 1985. Pp. xxv, 232. $27.00.

This volume consists of selected papers presented at a symposium held at UCLA in the spring of 1981. The title itself informs the reader that the net has been cast widely, and only the omniverous are likely to dine on the entire catch.

Of the six studies, only James F. Powers's "Two Warrior-Kings and Their Municipal Militias: The Townsman-Soldier in Law and Life" attempts a comparative study of Alfonso X of Castile (1252–84) and James I of Aragon (1213–76). The result offers a strong introduction to the military law and its sources in the thirteenth century and some indications of its different development in the neighbor kingdoms. Those with only a casual interest in medieval military practice or weaned on non-Hispanic sources for the same will be fascinated and instructed by the richness and detail of the *fuero* tradition indicated here.

Two other studies limit themselves entirely to the reign of Castile. Joseph F. O'Callaghan's "Paths to Ruin: The Economic and Financial Policies of Alfonso the Learned" provides a survey of the formal attempts of that monarch to come to grips with that hydra, the economy, largely as the former are recorded in the surviving *cuadernos* of the numerous meetings of the Cortes during the reign. What emerges is an instructive and entertaining description of sumptuary, forest, and grazing law; price, wage, and import-export regulation; and measures for control of the coinage, interest rates, and weights and measures. It will surprise no modern that the largely traditional, hapless "tinkering" of Alfonso failed to produce either a solvent monarchy or a coherent economy. At least on the evidence presented, one can cavil at the author's conception that an "economic crisis," distinct from the chronic penury of the crown, existed at the time.

Robert A. MacDonald's "Law and Politics: Alfonso's Program of Political Reform" makes explicit that the main lines of development in this regard had already been set by the time that "el Sabio" became king. He produced no major innovations in marriage, diplomatic, or military policy, all of which are outlined. The major novelty was a series of attempts to codify the law of the realm. The author labors manfully to clarify the purposes of that venture and does produce a review of the literature and the state of the major questions. Those familiar with the current progress of the study of Alfonso's legal reforms could hardly ask more.

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The three remaining studies treat of James I's Aragon, but in a rather different fashion. To my mind the most satisfying of the trio is Thomas N. Bisson's "Prelude to Power: Kingship and Constitution in the Realms of Aragon, 1175–1250." Of course, only one-third of this eighteen-page paper actually studies the reign of James himself. The major attention is given to the reigns of Alfonso II (1162–96) and Peter II (1196–1213). Such a procedure inevitably emphasizes the survey and introductory character of the offering, which is altogether innocent of the usual scholarly apparatus. Even so, few scholars are as well prepared as the author to undertake a current synthesis of the political and institutional realities of Catalonia and Aragon in the late twelfth and early thirteenth centuries. The halting developments of the royal administration, the tax policy it struggled to implement, and the resulting evolution of the *corts* of Aragon and Catalonia in response to these first two are firmly if broadly delineated.

Archibald R. Lewis, in "James the Conqueror: Montpellier and Southern France," chronicles the slow retreat of the former's power and influence in the Occitan before the stubborn advance of the Capetians. Essentially a sort of brilliant intuition, it calendars the still-considerable contributions of that region to James's successes and suggests that the king was groping his way toward an alternative model of rule based on commercial rather than territorial revenues and rights, the latter of which he found increasingly difficult to maintain. I am only prepared to leave the verdict on this thesis to those more intimately involved in divining the auguries.

Jill R. Webster's "Patronage and Piety: Catalan Letters from Llull to March" is an impressionistic essay on writers in Catalan into the fourteenth and fifteenth century. Although the author is obviously widely read, her writ runs elsewhere.

The editor, Robert I. Burns, provides a general introduction, introductions for most of the individual studies, a summation, a brief bibliographical essay, and an index. Three illustrations from the *Cantigas*, a genealogical table each for Alfonso and James, and one lamentably deficient map are also included.

BERNARD F. REILLY
Villanova University

ROBERT I. BURNS. *Society and Documentation in Crusader Valencia*. (Diplomatarium of the Crusader Kingdom of Valencia: The Registered Charters of Its Conqueror, Jaume I, 1257–1276, number 1.) Princeton: Princeton University Press. 1985. Pp. xi, 274. $40.00.

This is the introductory volume to what will be a series presenting the Valencian documents issued by King James I. Some twenty-three hundred records concerning every aspect of postconquest consolidation will be edited from the royal registers preserved in the Archive of the Crown of Aragon in Barcelona.

The volumes begin in the year 1257 and form the most extensive series of thirteenth-century European registers. They contain abridged versions of documents issued by the royal administration. For the most part, material for Valencia is interspersed with orders for James's other realms. The present volume is therefore of great interest and use not only for its description of the governance and society of Valencia but also as an introduction to the composition and bureaucratic function of the registers and how to evaluate them.

As Robert I. Burns's many previous works have shown, Valencia was an unusual case within the history of the Spanish reconquest. Unlike New Castile or Andalusia, Valencia's Muslim population remained a majority for generations after Christian conquest. Royal protection of the Muslims and Jews of this wealthy kingdom did not reflect an attempt at cultural harmony but stemmed rather from the king's practical method of exploiting what valor and fortune had brought him. Burns briefly but cogently depicts what he calls the "symbiosis" of different societies, characterized neither by confrontation nor by an idyllic *convivencia* but by a separation of cultures that, nevertheless, shared a certain commercial, courtly, and legalistic outlook and activity.

Subsequent volumes containing the edited charters will show how James administered Valencia and attempted to regulate the settlement of land, the resolution of disputes, military organization, religious practice, and the economy. The introductory volume summarizes such matters and increases the eagerness with which one awaits the appearance of the texts themselves. The first volume includes an extensive and magisterial account of the royal registers, ranging from discussion of the ink formula to the typology of documents included. Burns gives particular attention to how the availability of paper (especially from the famous mills at Játiva) encouraged production of elaborate bureaucratic instruments such as the registers. There are earlier paper volumes of notarial records from several parts of the county of Barcelona, but the legal, administrative design reflected in the royal registers was assuredly made possible by the conquest of Játiva and the preservation of its traditional industry. The expansion of the registers and the institutionalization of their use occurred once Játiva had been definitively taken over by King James.

Recent years have seen the realization of long-term efforts by American historians to describe the administration of medieval Catalonia and the Crown of Aragon. The *Diplomatarium* of Valencia

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will provide the first scrupulously edited and relatively complete set of royal documents from this realm. It represents a crowning achievement to Burns's research into medieval Valencia, and the present volume serves as an authoritative introduction to a massive and important project.

PAUL FREEDMAN
Vanderbilt University

BENJAMIN ARNOLD. *German Knighthood, 1050–1300.* New York: Clarendon Press of Oxford University Press. 1985. Pp. xii, 308. $34.50.

This book should be titled *The German Ministerialage*, because Benjamin Arnold argues that *ministerialis* rather than *miles* was the standard designation everywhere for an unfree knight in twelfth-century Germany. It is, in fact, the first modern general study in any language of the ministerials, an estate unique to that country. Arnold investigates such topics as the origins of the institution; the ministerials' legal rights, property holdings, military and administrative service, marriages, and propensity toward violence; the contradiction between their knightly functions and servile legal status; and their employment by the Salians and Hohenstaufen. Based on an extensive reading of primary sources and secondary works, this book is a useful introduction to a topic that has been largely ignored by non-German scholars.

Nevertheless, I have serious reservations about the book. First, Arnold stresses in his introductory chapter the autogenous rights of the nobility. Although this has been the dominant theory in postwar German historiography, some scholars, like Michael Mitterauer, have challenged it, and I have my own doubts about a theory that achieved canonical status during the Nazi era. Second, while Arnold rightly stresses the difficulties in discerning the origins of the institution, he virtually ignores Günther Flohrschütz's work on the origins of the Freising ministerialage and my own study of the emergence of the Salzburg ministerialage out of the *familia*. Third, any attempt to generalize about so complex a topic runs the risk of misconstruing local evidence. In the case of Salzburg, for example, Arnold calls an archiepiscopal retainer a Styrian ministerial (p. 143), attributes documents from the Abbey of St. Peter's to the cathedral chapter (p. 155), and cites a baseless fifteenth-century forgery to prove that ministerials had been buried in St. Peter's since 1139 (p. 157). More important, although Arnold denies it (p. 33), it is quite clear, as Otto von Zallinger demonstrated in 1878, that *miles* referred in twelfth-century Salzburg to the servile retainers of free lords and ministerials and to those retainers of a count who did not have their own vassals. It was not until about 1300 that prominent archiepiscopal ministerials were generally styled *milites* in sources originating within the archdiocese. This shows that ministerials cannot simply be equated with knights everywhere in Germany. As a result, Arnold misinterprets a number of sources from the archdiocese, for instance, the successive accounts of Archbishop Gebhard's return in 1086 that show the distinction between the ministerials and the knights (p. 35). I suspect that other regional specialists may be able to detect similar mistakes.

Finally, Arnold seems to accept Marc Bloch's argument that there was no such thing, legally, as a nobility before the twelfth century and that nobility was largely a matter of life style (pp. 69–70). For example, he links nobility with knightly descent (p. 136), but the Salzburg charter he cites merely states that the archbishop had granted the rights of archiepiscopal ministerials to the knightly families who belonged to the *familia* of the counts of Dornberg. Thus, while valuable, this book must be used with caution.

JOHN B. FREED
Illinois State University

JEAN-MARIE MOEGLIN. *Les ancêtres du prince: Propagande politique et naissance d'une histoire nationale en Bavière au Moyen Age 1180–1500).* (Ecole Pratique des Hautes Etudes, IV[e] Section, Sciences Historiques et Philologiques, fifth series, Hautes Etudes Médiévales et Modernes, number 54.) Geneva: Droz. 1985. Pp. xiii, 300.

The study of the idea of history embodied in the writings of premodern historians has long interested historians of thought, but until recently the substantive material of those histories, which seldom could stand up to closer investigation, has tended to be passed over in silence or dismissed with a condescending remark. Jean-Marie Moeglin has taken the trouble to unravel the ways in which the interaction of conflicting ideological interests managed by 1500 to produce a range of mythic patterns of the past in one German principality, Bavaria. The materials of a national history were singularly unpromising: although Bavaria had (and has) a strong awareness of its continued existence as a distinct "nation," its ruling dynasty, the Wittelsbachs, had only been raised from obscurity in 1180, when Frederick Barbarossa replaced the rebel Henry the Lion with the Palatine Count Otto I of Scheyern-Wittelsbach. What little light clerics of the High Middle Ages could shed on the antecedents of that house was hardly reassuring: the great twelfth-century chronicler Otto of Freising had gone out of his way to stress the treasonous habits of the pipsqueak counts of Scheyern-Wittelsbach, and in 1208 a Wittelsbach

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prince would shame his family by murdering the German sovereign Frederick the Handsome. It comes as little surprise that the genealogical table assembled by the ducal abbey of Scheyern sparked little enthusiasm and that Scheyern did not end up performing the propagandistic services St. Denis did so well for the Capetians in Paris. The histories that were actually written stressed the continuity of the Bavarians as a people and left the current ruling house virtually out of consideration.

Only in the last years of the fourteenth century, when the House of Wittelsbach had splintered into several branches, did the Scheyern material begin to serve as grist for a true dynastic propaganda. Tenuous ties with the Carolingians were magnified to give the Wittelsbachs a fictive claim to legitimacy in Bavaria well before 1180, and some chronicles even tried to interpret the whole of Bavarian state history as a function of Wittelsbach genealogy. In this way, the arbitrary exercise of power by the emperor in Bavaria was placed in a suitably negative light, since his interventions often limited ducal rights or even momentarily stripped the duke of his patrimony. By 1500, well before the beginnings of the career of the Bavarian humanist historian Aventinus, available models already ranged from national epics in which dynasties played almost no substantive role to dynastic chronicles in which the precise identity of the subjects of the ancient House of Wittelsbach seemed incidental.

Moeglin has produced a surprisingly well-written monograph on an unpromising subject, bringing together not only written but also visual materials. He has done extensive research in the manuscripts and printed materials of Bavaria. The book is a model for any future treatment of the exploitation of genealogy as a theme in medieval historiography.

STEVEN ROWAN
University of Missouri, St. Louis

GERHARD LOSHER. *Königtum und Kirche zur Zeit Karls IV.: Ein Beitrag zur Kirchenpolitik im Spätmittelalter*. (Veröffentlichungen des Collegium Carolinum, number 56.) Munich: R. Oldenbourg. 1985. Pp. 209.

Although criticized by his enemies because he gained election as a papal candidate and as the anti-king to Louis IV, Charles IV has long been recognized as an able diplomat who aggressively pursued the interests of the House of Luxemburg in Bohemia and the Holy Roman Empire. His thirty-two-year reign was most notable for the issuance of the Golden Bull of 1356, the elevation of Prague to metropolitan status, and his avoidance of the major conflicts of his day. Though educated in Paris, he identified closely with the Bohemian heritage of his mother, and it was as king of Bohemia that his reign has usually been judged most successful. It has been often said that Charles valued the empire only in dynastic terms and that he encouraged the trend toward territorialization and princely independence. Gerhard Losher's study of Charles's ecclesiastical policies in Bohemia and the empire provides an opportunity for a reassessment of this view. The author avoids extreme claims but sees Charles's effort to control ecclesiastical appointments as an instrument of establishing more effective imperial government.

Losher begins with a study of church policy in Bohemia, which he labels an attempt by Charles to build a *Landeskirche*. The cornerstone of this effort was the erection of Prague as an archdiocese in 1344, during his regency of the Bohemian kingdom and well before his election as German king. Charles, in cooperation with his old tutor, Pope Clement VI, succeeded in freeing Prague from the archbishops of Mainz. But Charles was not able to secure his complete goal, which would have seen Prague become the metropolitan see for Luxemburger territories outside of Bohemia. On his visit to Avignon in 1365, however, Charles succeeded in securing the status of apostolic legate for the archbishops of Prague and, with it, extensive rights in the German bishoprics of Regensburg, Bamberg, and Meissen. Efforts to extend the legation to cover Brandenburg failed, but the apostolic legation did strengthen Charles's hand both in the Bohemian church and in the German dioceses under its jurisdiction. In Losher's view, control of the Bohemian church was for Charles IV an essential element in the government of his kingdom.

The second part of the book deals with Charles's ecclesiastical policy in the empire, which Losher regards as parallel to his efforts in Bohemia. Although his initial successes in influencing episcopal appointments to imperial bishoprics derived from his close ties with Clement VI, this policy continued with some setbacks under succeeding pontificates until the end of his reign. Most notable were his efforts in the northeast, in the province of Magdeburg, and in the great Rhenish bishoprics. The chief thrust of his efforts were directed toward the latter, which constituted the heartland of the traditional German kingdom, and there his greatest successes were achieved. His detailed study of episcopal appointments in this region goes a long way in demonstrating both the direction and the success of Charles's imperial policies. Losher does not attempt to divorce these policies from their dynastic dimension or to present an argument that Charles was reviving dead dreams of empire. Rather, he places them in the context of the territorial *Politik* of the period. Students of the fourteenth- and fifteenth-

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century empire will find this volume valuable for its further insight into the empire under Charles IV.

JAMES M. POWELL
Syracuse University

M. AMELOTTI *et al. Per una storia del notariato meridionale.* (Studi Storici sul Notariato Italiano, number 6.) Rome: Consiglio Nazionale del Notariato. 1982. Pp. 374.

This book contains seven essays about notaries and their activities in southern Italy from the early Byzantine period through the fifteenth century. The authors have succeeded in unifying their work around the themes of how notaries supplied society with legally valid documents and how the services they provided fit into the social and economic development of this region. Notarial records have played an important role for scholars writing the histories of the northern Italian cities; for the first time, we now have a volume that surveys the extent and quality of the formidable mass of private documents that survive in the south.

Vera von Falkenhausen and Mario Amelotti consider Greek notaries and documents. The authors supply an invaluable bibliography of editions of Greek private records and demonstrate that the Byzantine parts of the south closely followed practices and legal changes emanating from Constantinople. The Norman conquest cut this area of Greek culture off from its roots, but, down to the fourteenth century, Greek notaries continued to redact documents in their own language and in an increasingly unique style. Southern Italy provides Byzantine historians with the bulk of private acts from the whole empire, but how this society endured into the Angevin period remains to be studied.

Mario Caravale's essay on the legislation of the Norman kings and the emperor Frederick II and how that legislation affected notaries and private business contracts is the longest and most important study in the volume. The Normans inherited a diverse group of subjects on the mainland and Sicily; Arabs, Greeks, Jews, Latins, and Lombards all had their own notaries, languages, legal heritage, and business traditions. Important cities such as Palermo and Naples retained and defended local customs. The Norman kings generally tolerated diversity and left the status quo intact. Frederick II wanted uniform practices to prevail throughout his kingdom. The well-known Constitutions of Melfi (1231) and subsequent additions contain numerous regulations concerning the notarial profession and contracts. Frederick reserved the right to appoint all notaries, excluded clerics and vassals from the profession, increased the duties of witnesses, subordinated the notaries to the royal judges, ended the use of archaic scripts, and declared decapitation the penalty for forgers. Caravale uses this legislation as a means to test Frederick's ability to implement his laws. The emperor made the notaries of the south a distinctive group that produced accurate and valid documents accepted throughout the kingdom, and his rules lasted into the Angevin and Aragonese periods. Naples and Amalfi, however, preserved much of their own local traditions intact; Greeks continued to follow Byzantine law, and clerical notaries practiced undisturbed in Sicily. The legal achievement must therefore be measured against the usual vagaries of practice.

Henri Bresc's essay on notaries in medieval Sicilian society highlights the unique cultural traditions on the island. Before 1150, few Normans and Latins lived in Sicily, so the bureaucracy and the notariat remained in the hands of Greeks and Arabs. Later, immigrants from the mainland or France and younger sons of the nobility entered the ranks of the notarial profession. The persistence of Arabic notaries in Sicily once again proves how durable local customs might be; their documents reflect the legal practices of Fatimid Egypt, and Jewish notaries used Hebrew letters to transcribe private acts in Arabic as well. Bresc also examines the social status of notaries by collecting data on their estates and the dowries they acquired by marriage. This profession had an honorable place in society and was a means of advancement for a few people of humble origins.

In his discussion of the notaries in the fifteenth century, Alfonso Leone makes the important point that historians should do a better job of integrating notarial evidence into broader works of social and political history and not simply treat the evidence as appropriate only to urban histories of particular towns. Leone notes that businessmen relied less on notaries because companies began to keep extensive private records. The Florentine Strozzi family had an important branch in Naples, and their records and others reveal that notaries were more and more confined to old-fashioned types of business. All the essays in this book break new ground and demonstrate how this difficult evidence can serve broader works.

STEVEN A. EPSTEIN
University of Colorado

A. P. KAZHDAN and ANN WHARTON EPSTEIN. *Change in Byzantine Culture in the Eleventh and Twelfth Centuries.* (Transformation of the Classical Heritage, number 7.) Berkeley and Los Angeles: University of California Press. 1985. Pp. xxii, 287. $35.00.

This book, the result of a fruitful cooperation between a historian and an art historian, challenges two widely held views: first, that Byzantine society

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and culture were fundamentally immutable and, second, that Byzantium, interesting though it may be, was esoteric, or at least peculiar, and so may be studied in a sort of splendid isolation from the rest of the medieval world. This book argues instead that Byzantine culture underwent significant changes in the eleventh and twelfth centuries and that its development during this period is indeed connected to the general evolution of the medieval world.

A. P. Kazhdan and Ann Wharton Epstein describe Byzantine society as undergoing a double process of urbanization and (incomplete) "feudalization." These two developments, although affecting the various parts of the empire with different force, influenced all aspects of political, intellectual, and cultural life. Among other things, the authors note that the urban populations and the aristocracy posed a double challenge to the authority of the central state. In the cultural sphere, they see the increased prosperity reflected in the variegated and relatively rich food and dress; urbanization and feudalization resulted in the emergence of both popular and aristocratic elements in art and literature—a phenomenon that they identify as one of the elements differentiating Byzantium from Western Europe. It is an interesting argument, which, one hopes, will generate discussion, especially from Western medievalists.

The Byzantines had, throughout their history, a complex relationship with the classical past. The authors suggest that intellectuals and artists in the eleventh and twelfth centuries had so assimilated the classical heritage that it formed an integral part of both the conceptualization and the form of their activities. Kazhdan and Epstein then trace the appearance of a new interest in humanity and in the individual in art and literature. Theology is also treated as an integral part of culture. The sections of the book that deal with theology, with literature generally, or with specific authors are among the most illuminating and original. An excellent selection of extracts from sources, some well known, some quite unusual, completes the book and enhances its value.

This is a synthetic work, heavily dependent on previous studies by the authors themselves and by other scholars. But, in the manner of all good synthetic works, the sum is greater than the parts, and the originality lies in the fact that the total picture that emerges is quite different from the standard one. It is a rich book, full of ideas and new interpretations that will inevitably provoke discussion. One may argue that the relationship between the urban populations, the aristocracy, and the state was more complex than what is presented here, especially when one bears in mind a recent reinterpretation of the social structure of eleventh-century Byzantium, which shows that there was, indeed, a potentially dynamic merchant and artisan class whose development was stifled by the Comnenian dynasty, that is, by the aristocracy that had captured the state. One may further object that the weakness of structures such as the guilds is exaggerated here; again, in the eleventh century there is evidence that guild activity was, arguably, more important than the authors suggest. The general argument that the only "microstructure" with vitality in Byzantine society was the nuclear family is one that must be further discussed and elaborated; such a conceptualization would have to incorporate an explanation of the fact that in Byzantium the dissolution of the nuclear family through divorce was a possibility, whereas in Western Europe it was not. Finally, it is rather intriguing that the authors posit an almost complete coincidence between developments in art and developments in every other aspect of life and culture. What underlies this is a very healthy intent to use art as a historical source. What is less clear, at least to readers who are not art historians, is the extent to which the artistic material might also incorporate other, different, and perhaps opposing tendencies. It should be noted in this connection, however, that, in drawing their total picture, the authors depend heavily on provincial art, a refreshing and, for this period, most appropriate choice.

Arguments and discussion must, of course, be expected by the very nature of this original and sometimes provocative work. Its great merit is that it is the most comprehensive and coherent study of a period of cardinal importance not only for the Byzantine empire but also for the European Middle Ages. The book merits an important place in the literature on the twelfth century. It should be read by medievalists as well as Byzantinists, and its publication makes it possible now to discuss the Byzantine empire as an integral part of the eleventh- and twelfth-century world, a discussion that should enrich our understanding both of Byzantium and of medieval Europe in general.

ANGELIKI E. LAIOU
Harvard University

MODERN EUROPE

EUGENE F. RICE, JR. *Saint Jerome in the Renaissance.* (Johns Hopkins Symposia in Comparative History.) Baltimore: Johns Hopkins University Press. 1985. Pp. xii, 289. $24.00.

Ascetic, scholar, moralist, critic, biblical translator, and exegete, Saint Jerome of Stridon left one of the most varied legacies of all the Latin church fathers. It is little wonder, then, that he appealed strongly

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and in numerous ways to the medieval and Renaissance imaginations. In this elegant and rewarding volume, Eugene F. Rice, Jr., charts the continuing influence of Jerome on Christian Europe. He considers materials from the patristic age through the seventeenth century but concentrates on the period from the early fourteenth through the mid-sixteenth century, when Jerome's example proved most suggestive for the later experience of Western Christendom.

The strength of this book derives from the author's broad-gauged approach to his subject. Spirituality, theology, scholarship, biblical studies, and iconography all figure prominently in Rice's analysis. He shows that, beginning in the late thirteenth century, reformers drew inspiration from Jerome's asceticism and his legendary devotion to the Virgin; as a result, Hieronymite cults flourished, especially in Italy and Spain, for the next three centuries. During the fifteenth century, Italian humanists admired Jerome's eloquence, knowledge of languages and literature, moral commitment, and penetrating biblical scholarship; the humanists had little urge to imitate Jerome's asceticism, but they eagerly invoked his example in defending their own efforts at scholarship and in justifying their ideal of uniting wisdom with eloquence. Rice devotes a separate chapter to the special case of Erasmus, the most discerning of all the humanist admirers of Jerome. Rice attempts to avoid an exaggerated characterization of Erasmus as a thoroughly modern scholar, but his argument demonstrates clearly that Erasmus's work constituted a turning point in the understanding of Jerome. Erasmus prepared the first reliable edition of Jerome's letters, offered philological and historical explanations of difficult passages, and presented a remarkable biography that extracted the historical Jerome from the numerous improbable legends that had accreted to his legacy during the Middle Ages. The Reformation brought complications for Jerome's reputation. Associated with the Vulgate Bible, asceticism, monasticism, Mariology, orthodoxy, and papal primacy, Jerome had little appeal for Protestants, although Catholic Counter Reformers found him useful as a skilled and spirited polemicist on behalf of orthodox views. Throughout his work, Rice pays close attention to the developing iconography of paintings and engravings of Jerome, demonstrating that the artistic imagination reflected rather accurately the broader social, intellectual, religious, and spiritual appeal of the saint.

Rice's study complements the more specialized works of Charles Béné, André Godin, Charles Stinger, John Olin, and others who have investigated the Renaissance revival of interest in patristic antiquity. Most other scholars have concentrated analysis on a single Renaissance humanist. Rice's work demonstrates the high value of an alternative approach that traces the fortunes of a single church father through time, and it suggests the desirability of parallel studies devoted to other figures, beginning with Saint Augustine.

JERRY H. BENTLEY
University of Hawaii

JAMES M. KITTELSON and PAMELA J. TRANSUE, editors. *Rebirth, Reform, and Resilience: Universities in Transition, 1300–1700*. Columbus: Ohio State University Press. 1984. Pp. 367. $25.00.

Although the study of universities as a historical specialty existed far back into the nineteenth century, it long tended to be a rather narrow field, sunk in the depths of local antiquarianism, filiopietism, and the most arid sort of institutional research. Its products were often mere compilations of data, useful chiefly as works of reference for scholars active in livelier, more intellectually challenging specialties. The reader of many older university histories would hardly suspect that any ideas or even any significant educational activities existed in these institutions or that universities had any direct and meaningful role in the general history of the societies within which they existed. In recent decades, however, historical study of universities has entered the mainstream of scholarly research.

The present volume of essays, an outgrowth of a conference held in 1979 at the Ohio State University, is a good example of many of the new directions in university history. Unlike local antiquarians who narrowly concentrate on institutional structures, the authors in this collection have demonstrated the university's intimate contacts with the cultural, political, and social history of late medieval and early modern Europe. Each essay has something of value to say about its particular subject, but far more broadly significant is the repeated demonstration that the universities of this period were not remote from the realities of their age, not decadent backwaters reduced to relative insignificance by great historical developments such as the Renaissance, the Reformation, the development of strong territorial states, the growth of early capitalist society, and the rise of modern science. Although the relationship of these medieval institutions, the universities, to the historical developments of the period 1300–1700 was frequently awkward and always complex, this collection of papers demonstrates that the universities played an active, creative, positive role within their societies.

Following a thoughtful introductory essay that James M. Kittelson enriches with examples from his own research into the history of the Strasbourg Academy, two general papers by Heiko A. Ober-

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man and Lewis W. Spitz challenge the traditional opinion that for the universities the late medieval and Reformation periods were a time of declining importance when these institutions lost their sense of purpose and either became irrelevant to the main lines of historical development or allowed themselves to be prostituted by the embrace of authoritarian princes.

Sooner or later (as I can attest from my own experience) any serious student of intellectual history in this period has to consider at length the role of the universities in the history of ideas. Although at times the universities did seem to play the obscurantist role attributed to them by humanist satirists such as Erasmus and the authors of the *Letters of Obscure Men*, mature reflection produces a more complex picture that attributes to the universities a positive, active share in the growth of early modern culture. The papers of Edward Grant, William J. Courtenay, and Paul W. Knoll in different ways show that late medieval academic institutions were fully engaged in intellectual developments (such as the study of natural science, the spread of fresh philosophical and theological ideas from fourteenth-century England to other parts of Europe, and the effort to reform the church and redefine the relationship between papal authority and the authority of councils) that represent the very opposite of the intellectual exhaustion and irrelevance traditionally attributed to them.

The way in which universities were directly and creatively involved in the political and social life of their time and place constitutes the principal subject of Knoll's essay on fifteenth-century Cracow, John M. Fletcher's reevaluation of the importance of university migrations in the late Middle Ages, Guy Fitch Lytle's study of the careers of Oxford students, and James M. Overfield's treatment of the education of the clergy in pre-Reformation Germany.

Less obviously linked to the history of universities than these chapters is M. A. Screech's essay on the attitude of Erasmus and Rabelais to the study of Hebrew. Both discussions, however, are linked to the University of Paris by the involvement of the two humanists with the archconservative Noel Beda, syndic of the Sorbonne. Screech clarifies in a convincing way the attitude of Erasmus to the study of Hebrew, demonstrating how Hebrew shaped the Dutch humanist's approach to interpretation of the New Testament. He also provides a thoughtful analysis of Erasmus's reservations about the occultist side of Renaissance Neoplatonism, especially the cabalistic enthusiasms of Johannes Reuchlin. The volume concludes with a bibliographical essay by J. M. Fletcher and Julian Deahl, who discuss and list recent works that deal with the history of universities in the period under study. Although this collection of papers is certainly no substitute for the still-unwritten synthesis of the whole subject, it is far more unified and far more uniformly valuable than are most publications of conference proceedings. It is regrettable that so useful and carefully researched a collection should be marred by a great number of typographical errors that a careful proofreading would easily have eliminated. The authors deserved better of their publisher.

CHARLES G. NAUERT, JR.
University of Missouri

A. C. DUKE and C. A. TAMSE, editors. *Clio's Mirror: Historiography in Britain and the Netherlands*. (Eighth Anglo-Dutch Historical Conference; Britain and the Netherlands, number 8.) Zutphen, the Netherlands: De Walburg. 1985. Pp. 238. f. 39.50.

Space precludes any comprehensive discussion of the individual essays in this book edited by A. C. Duke and C. A. Tamse. All are contributions to historiography, and all are important reading for those working in European or British history. The significance of the "Trias Historica"—that is, the writings of Emanuel van Meteren, Pieter Bor, and Everhard van Reyd—and of Hugo Grotius and P. C. Hooft to the historiography of the Revolt of the Netherlands and to an understanding of their own times is skillfully handled. Especially illuminating is the relative tolerance in violently partisan times shown by these pioneers in the writing of Netherlands history.

Gerard Brandt and Gilbert Burnet, though more biased, changed "church history" to "Reformation history." Brandt may have been the first historian to use the term "Reformation" to refer to a period of history rather than to a theological struggle. The veracity of John Foxe's *Book of Martyrs* is questioned, especially Foxe's tendency to ignore the axiom of his time that the cause, not the death, made the martyr.

Able Boyer was a political journalist turned historian, and writers from Voltaire to Richard Price, from Schiller and Goethe to John Andersen were very much influenced by Dutch events in the sixteenth and seventeenth centuries. During the two succeeding centuries there was a tremendous interest throughout Europe and America in the history of the Low Countries.

Robert Fruin, "the Dutch Ranke," illustrated in his writings that the history of the Netherlands rested more on humanist and classical foundations than on romanticism. P. S. Blok wrote on the history of the Dutch people, and Pieter Geyl wrote about the history of the Dutch-speaking people. The attitudes of these three scholars on liberalism, union with Flemish Belgium, and cultural and constitutional matters are reflected in their treatment of the clashes in the sixteenth and seventeenth centuries

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between the House of Orange and the Stadthouders. L. de Jong outlines post–World War II historiography in the Netherlands.

Members of history departments will feel at home as they follow the struggles of Sir Charles Firth and other professional historians with "the Gang" at Oxford who contributed little or nothing to historical knowledge. On one point, however, I take issue with the author. I was a student of Godfrey Davies and found him to be an excellent teacher. The essay on Firth and the teaching of history at Oxford is delightful reading and tells much about feuding historians there. Of pertinent interest is the listing of the large number of American historians who came to Oxford to study under Sir Charles.

JOHN J. MURRAY
Coe College

PIETER SPIERENBURG. *The Spectacle of Suffering: Executions and the Evolution of Repression; From a Preindustrial Metropolis to the European Experience.* New York: Cambridge University Press. 1984. Pp. xii, 274. $39.50.

Pieter Spierenburg's subject in this provocative work is the gradual disappearance of publicly executed punishments in early modern and modern Europe. His ambitious objective is to propose an alternative to Michel Foucault's paradigm of penal evolution.

Spierenburg examines the evolution of penal practices in terms of changing mentalities and state development. Building on the work of Norbert Elias, he maintains that Europeans, as they became more sensitive to human degradation, increasingly disapproved of public punishments. According to Spierenburg, the emergence of the modern nation-state also contributed to the disappearance of public executions. These spectacles, necessary reinforcements of the relatively unstable early modern state, became unnecessary with the emergence of the stable, modern nation-state.

Spierenburg uses Amsterdam as a case study by which to prove his ideas while drawing on other European examples as well. He explores the roots of the European criminal justice system in the twelfth century and examines popular attitudes toward public executions as reflected in the actions of spectators at these events and in their general contempt for executioners. The author also discusses the execution day ceremonies designed to reinforce the power of authorities. The core of Spierenburg's study, chapter 5, draws on the excellent series of sentence records for the period 1650–1750 in Amsterdam's Gemeente-archief to illustrate his theory of penal development with quantitative evidence drawn from 2,991 public executions and from a sample of nonpublic sentences.

Spierenburg's research in these Amsterdam records demonstrates a gradual erosion of the spectacle of public punishment rather than the sudden, late eighteenth-century change in penal practices posited by Foucault. The Amsterdam records reveal an end to the mutilation of convicts in the seventeenth century, followed later by the abolition of torture and the exposure of corpses and finally by the cessation of all public punishment in the nineteenth century. Spierenburg's knowledge of scholarship on the English, French, German, and even the North American penal experiences is impressive, allowing him to apply his pattern of Dutch penal development to all of Western Europe. But the author's attempt to prove that these developments were the result of changing sensibilities is not entirely convincing. He concedes that further research is necessary elsewhere in Europe to sustain his conclusions. But even for his Dutch case study one might wish that the author had made a stronger case for a growing sensitivity to human degradation as the chief cause of the new penal practices. Is there, for example, evidence of concern for convicts in reform literature that could also be cited to prove the author's ideas?

These points suggest that additional research is necessary before Spierenburg's paradigm supplants that of Foucault, but they do not diminish the importance of this work. It is a major effort to move criminal justice history beyond Foucault and demands the attention of all scholars in the field.

JULIUS R. RUFF
Marquette University

PAUL M. HOHENBERG and LYNN HOLLEN LEES. *The Making of Urban Europe, 1000–1950.* (Harvard Studies in Urban History.) Cambridge: Harvard University Press. 1985. Pp. xiv, 398. $30.00.

This book aspires to provide a "history of the way Europe urbanized." It attempts to implement the increasingly influential idea that the proper framework for urban history is the study of urbanization. But how can such a history, which spans a millenium, be presented in a single volume intended to have both a pedagogical and a research function? Lynn Hollen Lees and Paul M. Hohenberg seek to accomplish their mission by organizing their material around a few key concepts.

The book is divided into three unequal parts: medieval, early modern, and industrial urbanization. The authors' most important concepts are two models of urban systems and a cyclical model of urban growth. Hohenberg and Lees use central place theory to make sense of the spatial and functional characteristics of Europe's cities as they emerged in the Middle Ages and developed there-

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after. But the authors do not stop here. Noting glaring deficiencies in central place theory as a description of the European urban settlement pattern, they go on to develop a second approach that is intended to be complementary to the first. Thus, to the concept of a "central place system" of cities (fashioning a hierarchy based on control of hinterlands and local trade) Hohenberg and Lees add the idea of a "network system" of cities, a more flexible, long-distance, trade-based constellation of cities whose chief linkages are to distant places and regions rather than to their local hinterlands. Although they are not the first to make this distinction, they develop it more fully than others have and explore its political and cultural as well as geographic and economic implications.

By asserting the existence of two distinct, but overlapping, urban systems, Hohenberg and Lees succeed in covering most historical patterns of urban growth. If a city does not fit one model or the other, it may fit into the large class of cities that participated in both systems. The authors identify many other instances of dualism in their review of urban history. Medieval cities were an integral part of feudal society but also grew apart from it; autonomous cities acted to thwart nation-state formation, except where cities were integrated into such states and became agents of consolidation. Urban demography was comprised of permanent and "floating" populations; cities were characterized by both their insularity and internationalism.

With these and other examples of dualism, Hohenberg and Lees manage to bring some order to their subject, with its thousands of cities and wide time span. Their efforts are sometimes vitiated by the breathless recitation of examples, exceptions, and caveats and the introduction of a clutter of topics for which there is no space for adequate development. All of this is presumably intended to add historical life to what might be deemed an otherwise overly social scientific approach. Through the mass of detail several important arguments emerge that make this volume of special interest to urban historians. They identify the great achievement of medieval urbanism not in the rise of the autarkic merchant city but in the transformation of Europe's nascent urbanism from an appendage of eastern Mediterranean centers into the core of an independent European urban system. In their view this occurs neither in the Industrial Revolution nor in the early modern period but in medieval times.

From the fourteenth through eighteenth century, urbanization proceeded through cycles of a type long familiar to students of B. H. Slicher van Bath, Wilhelm Abel, and Emmanuel Le Roy Ladurie. Hohenberg and Lees give an urban application to the interplay of population, agricultural production, prices, and wages and conclude that agricultural productivity limited urban expansion in this era, imposing a cyclical interlude between the linear character of both medieval and industrial urbanization.

In their treatment of modern urbanization Hohenberg and Lees go to great lengths to unravel the complex of urbanization, modernization, and industrialization. Industrial technology contributed directly to urban growth only at a late stage in its development, and it transformed the urban hierarchy only in isolated regions. In general, the authors seek to qualify the more sensational claims about the uprooting, degrading, alienating character of modern urban growth.

These and other arguments advanced by Hohenberg and Lees in their analysis of a thousand years of European urbanization are not immune to criticism, but none of them is made idly. Each chapter reflects wide reading and thoughtful reflection. This volume will be of use to everyone interested in urban history.

JAN DE VRIES
University of California, Berkeley

NICHOLAS BULLOCK and JAMES READ. *The Movement for Housing Reform in Germany and France, 1840–1914.* (Cambridge Urban and Architectural Studies, number 9.) New York: Cambridge University Press. 1985. Pp. ix, 657. $79.50.

The title of this significant and skillfully illustrated book is misleading in two regards: it blurs the true focus of research, which is the question of public housing in the urban centers of Berlin and Paris, only tangentially in all of Germany and France, and it omits an implicit but important theme (on which both authors insist) of British influence on social reformers of continental Europe in the late nineteenth century.

The story is nonetheless ably recounted—by Nicholas Bullock for Germany and James Read for France—in two parallel sections, each with three identically titled parts. The collaborative effort is thus manifest, yet the respective approaches are not altogether compatible. Bullock pays closer attention to the building trade as such during the *Kaiserreich*—architectural design, loans and mortgages, land values and banking practices—whereas Read's account centers primarily on the pressure groups and tortuous parliamentary process of the early Third Republic, thereby recording the repeatedly frustrated attempts to achieve legislative action on the housing problem.

Although the resulting book is ostensibly written by and for architects, it deserves careful notice from social historians, who will not fail to observe two

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familiar and fundamental distinctions, as well as one basic similarity, between France and Germany. The differences, amply documented by Bullock and Read, derived from faster demographic growth in Germany, which had an accordingly more rapid pace of industrialization and urbanization (Paris, of course, excepted), and also from Germany's stronger federal and civic traditions, which encouraged greater municipal autonomy than in France. Despite these essential divergences, however, one common denominator remained: the failure of private enterprise, on which the early liberal reformers in both countries literally banked, to provide adequate public housing for the poor. Consequently, the period before the First World War was one of disappointed expectations rather than of solid achievements. The era of accomplishment through concerted state intervention still lay in the future. Hence, if ever a historical work obviously required a sequel, this is it.

In addition to the intrinsic merits of their study, Bullock and Read must be commended for providing a possible model of cooperative history. No doubt "cooperative" is the proper word, instead of "comparative," because of certain incongruities in style and emphasis. Nevertheless, this effort may still set an example for others of one way to escape the narrow confinements of national history.

ALLAN MITCHELL
University of California, San Diego

C. B. A. BEHRENS. *Society, Government, and the Enlightenment: The Experiences of Eighteenth-Century France and Prussia*. New York: Harper and Row. 1985. Pp. 248. $20.00.

Although historians will always argue about the relative importance of various changes wrought by the French Revolution, few will deny major significance to the establishment of the principle of equality before a law that was the same for all. Negatively put, this meant the abolition of the *société d'ordres* (*Ständegesellschaft*), a society common to most of Europe and composed of privileged groups for whom the law was differently written. This kind of society posed serious problems for economic modernization and military growth everywhere, but in only one country—France—did a revolution become necessary to overcome these problems. By comparing and contrasting the experiences of eighteenth-century France and Prussia, C. B. A. Behrens attempts, in this fascinating and important book, to discover why that was so and, in the course of that attempt, to illuminate the root causes of the revolution itself.

Her answer lies essentially in the different relations between the governments and the various social orders of the two states. She argues that the French monarchy in the eighteenth century had to such an extent become hostage to the multiple expedients by which it had raised itself to absolutism within a *société d'ordres*—including but not limited to the dangerous venality of office and of noble status—that it had unfitted itself to discharge its primal social tasks, as those were then defined. The monarchy thus increasingly forfeited its various constituencies to the point where its acceptance, weakened further by ideological alternatives put forward by the Enlightenment, fell below the minimum needed to sustain it. In everything from taxation and fiscal administration to justice and the promotion of wealth, welfare, and military power—to all of which Behrens devotes considerable detail—the record was a woeful one. Indeed, the government and society of France were virtually at war within and between themselves well before the revolution, giving the picture of a regime that "had fallen to pieces before the revolutionaries attacked it" (p. 175) and suggesting perhaps that what was sought in 1789 was less a change in government than government itself.

In a parallel analysis of Prussia's government and policies, Behrens makes clear not only that the essential tasks of government were more efficiently and successfully executed than in France but also that this was commonly perceived to be so within Prussian society at all levels. Frederick William I and Frederick II, for all their differences, were able to impose on their society a unity of effort and at least the appearance of equal sacrifice in the service of the state, which did much to elevate the ideal of public service over private interest and to reduce the disruptions caused by social envy. They were thus able to create and sustain a social consensus whose absence was fatal to the French monarchy. The German Enlightenment, furthermore, attracted by both the attitudes and much of the practice of Frederick II, served on the whole to strengthen this consensus in Prussia and to act as a unifying force, in strong contrast to its disruptive role in France. Finally, Behrens argues, Prussian statism, in both ideology and practice, came to embody so much that was "bourgeois" already that it was able not only to spare Prussia revolution but also, after 1807, to eliminate by peaceful reform those formal aspects of the *société d'ordres* that still presented obstacles to economic and military development.

JOHN G. GAGLIARDO
Boston University

ROBERT GASCOIGNE. *Religion, Rationality, and Community: Sacred and Secular in the Thought of Hegel and His*

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Critics. (International Archives of the History of Ideas, number 105.) Boston: Martinus Nijhoff. 1985. Pp. xiv, 308.

The story of Hegelianism has been frequently told, and the various interpretations divide into schools of the Left, Right, and Center. Sidney Hook, Herbert Marcuse, and David McLellan followed the evolution from Hegel to Marx from a left-wing perspective, whereas Lukács, in *The Destruction of Reason*, presented a critical, Left-Hegelian version of the right-wing inheritance. Karl Löwith believed that Hegelianism separated into a right and left and that nineteenth-century Hegelianism terminated in the right of Nietzsche; the recent work of John Edward Toews adopts a more neutral analytic position. Robert Gascoigne's book traverses this familiar territory from a refreshingly different point of view: he pursues Hegelianism but from a right-wing framework.

Assuming a strong religious emphasis in Hegel's later writings, Gascoigne understands the Hegelian synthesis as proposing a conjuncture between the divine and the human. This philosophy of immanence, in which subject and history (Gascoigne sees this as an aspect of religious spirit) were joined, broke apart under the pressure of Bruno Bauer's critical self-consciousness. The remainder of the post-Hegelian radicals—Feuerbach, Hess, Ruge, Cieszkowski, and Marx—all shared the idea that history was a process in which man could attain his own emancipation. Gascoigne locates the origin of the Hegelian right in the idea that man cannot construct his own redemption in history. Lukács found the inception of right-wing Hegelianism in Schelling's irrationalism, whereas Gascoigne situates it in Schelling's Kantianism, the inability of theoretic reason to comprehend the universe. For Gascoigne, Kierkegaard stands as the end of the Hegelian project, for existentialism begins with the negation of the philosophy of identity, in the belief that man and history are eternally severed and that it is only in subjective inwardness that humanity can find authenticity.

Gascoigne correctly recognizes that the problem of man's relation to history stands as the line of demarcation between the Hegelian left and right, the political radicals as opposed to the political conservatives. Those who abandoned the idea that human *praxis* can be a constitutive force in history generally fall into a negative dialectic, surrender the idea of any human-cultural integration, and conclude that man can find peace only in subjective cultivation. Political activism can only arise from the belief that human *praxis* is a force of practical-revolutionary modification.

My one major criticism of this book is its excessively descriptive nature. Gascoigne has a viewpoint, but he restrains it to an unnecessary degree; what emerges is a narrative of the thoughts of the great men of the Hegelian movement. Nevertheless, it is enlightening to read a portrayal of this significant nineteenth-century cultural movement in which the hero is not Marx but Kierkegaard. We have a right-wing analysis of the Hegelian phenomena, and Gascoigne's portrayal offers a needed change of perspective as well as textual insightfulness.

NORMAN LEVINE
University of Maryland, Baltimore

SANDER L. GILMAN. *Difference and Pathology: Stereotypes of Sexuality, Race, and Madness*. Ithaca: Cornell University Press. 1985. Pp. 292. Cloth $34.95, paper $12.95.

In ten essays, five of which have appeared before, Sander L. Gilman explores images of women, blacks, and Jews in late nineteenth- and early twentieth-century Europe. His evidence comes primarily from literary and medical sources, although he also draws on painting. Most of the essays seek to link women, blacks, and Jews to ideas about sexual perversity and mental illness. They also show how these projections flow into one another (women becoming identified with blacks, Jews with women) to form a generalized, alienated conception of the Other.

The studies are unfailingly inventive, often suggestive, and sometimes even persuasive. In one of the more incendiary (but still representative) essays, for example, Gilman argues that the nineteenth-century image of the prostitute became conflated with that of the black. Both, he contends, were perceived as oversexed, a quality distilled, so to speak, in their supposed tendency to plumpness and, above all, in their large rumps—or "steatopygia" (p. 99), to use the scientific jargon of the day. He illuminates this connection by juxtaposing Manet's ample representation of the prostitute Nana with drawings and descriptions of Sarah Bartmann, the so-called Hottentot Venus. Although we may not fully embrace his conclusion that the prostitute and the black had merged in the European unconscious, he nonetheless leaves us with the firm impression that he is onto an important affinity.

All racial and sexual stereotypes, in Gilman's view, result from projection. They reflect anxieties about the integrity of the self. Following the object relations school of psychoanalysis, he suggests that the stylized representation of the Other is essentially an externalization of the bad self, undertaken in an effort to maintain control over the world. In the late nineteenth and early twentieth centuries, these anx-

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ieties were projected onto women, blacks, and Jews because, by demanding equality, they were upsetting the psychic equilibrium of the dominant classes. One wonders whether a similar mechanism could be identified among the aristocrats of the Old Regime as they braced themselves before the political and social onslaught of the European bourgeoisie. Is there, in other words, an eighteenth-century stereotype of the bourgeois corresponding to the nineteenth-century stereotype of the woman, the black, and the Jew?

The principal weakness of Gilman's book is that it begs questions of historical fact. To be sure, he concedes that stereotypes "are rarely without some point of tangency with reality" (p. 240), but characteristically he insists that such manifestly bogus ideas can be explained only through psychological processes. The assertion would carry more weight if he were able to show that the images in question had no empirical basis. But the historian reading these essays is bound to wonder, with typically cranky waywardness, whether European prostitutes in the nineteenth century did not perhaps have large rumps after all.

PAUL ROBINSON
Stanford University

NORMAN RICH. *Why the Crimean War? A Cautionary Tale*. Hanover, N.H.: University Press of New England, for Brown University, Providence, R.I. 1985. Pp. xix, 258. $20.00.

As its title indicates, this book attempts something that will make some historians suspect a presentist bias, moralizing, or worse. Norman Rich recounts the political history of the Crimean War from its origins to the final settlement expressly in order to indicate its lessons for the present day.

Be reassured: the book succeeds. First, it represents genuine scholarship, drawing on most of the important published documentary and secondary sources, including recent works. It is also real history, not didactic moralizing or pseudopolitical science. The major developments are clearly narrated and explained, and the central questions are judiciously weighed and answered. If, as is inevitable, many interpretations remain debatable, Rich always argues plausibly and gives his evidence. I do have some disagreements (one of which he generously discusses). Basically, he seems to me to overstate the impact and folly of Austria's anti-Russian stand in early and mid-1854 and to underrate what Austria did to challenge Russia later, so that the foreign minister Count Karl Ferdinand Buol-Schauenstein is made at one time more belligerent and at another more desperate for peace than he really was. But these and other questions that might exercise historians are not critical for the overall picture Rich draws or the audience he has in mind. Moreover, the book is well written and is easily the best work on the subject for the undergraduate or general reader.

Likewise, this book is important for historiography. Although Rich does not claim novelty, the main points he makes—that the war was not accidental but caused, and caused most importantly by the Anglo-French determination not to allow Russia a peaceful retreat; that the Anglo-French decision to insist on a military victory for reasons of prestige rather than a political settlement unnecessarily prolonged it; and that the peace settlement did nothing to replace the concert of Europe that the war had wrecked—are entirely sound and ones on which, one would hope, this book will help establish a consensus.

Does Rich provide a clear moral for today, as he hopes? Yes and no. On the one hand, he is eloquent and convincing in arguing that confrontation as the primary way of dealing with a threatening power, counterproductive already in the nineteenth century, is intolerably dangerous now and in advocating the post-1815 method of "grouping" dangerous states by concert diplomacy. On the other hand, he not only fails to explain just how "grouping" would work today (which one cannot expect him to do) but also, it seems to me, fails to indicate precisely how grouping worked in the nineteenth century. Yet its two central principles, and the results of violating them, are clearly indicated by the Crimean War and implicit in Rich's account: first, always to interpose an international community (here, the concert of Europe) between the dangerous power and its potential target; second, always to try to restrain a dangerous power not primarily with countervailing power or a hostile coalition but through its friends. How the strategy of "grouping" might be applied to today's world cannot be discussed here; that it should be, I fervently agree.

PAUL W. SCHROEDER
University of Illinois

WALTHER HOFER. *Mächte und Kräfte im 20. Jahrhundert: Gesammelte Aufsätze und Reden zum 65. Geburtstag*. Edited by PETER MAURER. Düsseldorf: Droste. 1985. Pp. 264. DM 38.

The Swiss historian Walther Hofer is outstanding for his combination of distinguished scholarship and public service. Starting his career at the Free University in besieged West Berlin, he subsequently held a professorship at the University of Bern while also serving in the Swiss parliament and, after 1967, in the Swiss delegation to the Council of Europe.

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The present volume, published in honor of his sixty-fifth birthday, presents a selection of essays, speeches, and comments covering the wide range of subjects on which he speaks with authority. The subjects covered include the East-West conflict in historical perspective, National Socialism in World War II (including the role of neutral Switzerland), European security from the Locarno Pact of 1925 to the present, Europe and the German question, the relations between domestic and foreign policy in Switzerland, and, most emphatically, freedom and human rights, a subject close to his heart. The quality as well as the tone of these essays derive from his maxim that "the present condition of the world compels us to examine the basic questions of our existence as never before. This applies to the global as well as to the European framework of international relations" (p. 37). A well-informed specialist on German and European history and at the same time a generalist willing to examine the foundations of our existence, Hofer takes a refreshingly broad view. Would that there were more historians with that gift!

One might disagree, however, with the political convictions wrapped up, or sometimes sharply accentuated, in these pages. Hofer is a strongly anti-Soviet conservative, the proud representative of Western political faith. His limitations show up most clearly in his discussion of human rights. He quotes Julius Nyerere's claim that a full stomach takes precedence over human rights and daringly asks whether human rights and freedom are merely a luxury of the rich. But he shrinks at once from such treacherous relativism, convinced that these values constitute transcendant ideals applicable everywhere and under all conditions.

This claim to universality casts some doubts on the depth of Hofer's historical understanding. Historical analysis should explore the causes of phenomena such as human rights. What were the conditions responsible for their rise in Western Europe and the United States? Do the same conditions prevail in the Soviet Union and the developing countries? And what of the most important human right, the one most keenly felt by the have-nots but typically unnoticed in the expansionist West: the right to live in a secure and respected polity? Of such considerations, so crucial in this age of global interdependence, we find no trace in these pages. Obviously, from the peaks of Swiss righteousness the global perspectives extend no further than Switzerland and Western Europe.

And yet Hofer's essays deserve to be read by American scholars interested in German and European history. Hofer belongs among the distinguished contemporary European historians, both as a scholar and a public figure. Despite his Eurocentric parochialism, he sets a model of how history in our age should be treated.

THEODORE H. VON LAUE
Clark University

JEAN-PIERRE MOREAU. *Rome ou l'Angleterre? Les réactions politiques des catholiques anglais au moment du schisme, 1529–1553.* (Publications de l'Université de Poitiers, Lettres et Sciences Humaines, number 22.) Paris: Presses Universitaires de France. 1984. Pp. 377. 190 fr.

The drama of the English Reformation has made it one of the few historical epochs that still appeal to the general public. The murderous Henry VIII, stubborn Catherine of Aragon, Anne Boleyn, Thomas Cardinal Wolsey, Thomas Cromwell, Thomas More, Thomas Cranmer, Mary Tudor, and the great Elizabeth stride with many another across the stage of our collective memory, creating endless excitement no matter how many times we have seen the play and pondered its lines.

Yet the meaning of the drama has been obscured by the secular preoccupations of the best modern historians of the period. It is a civil blessing that the theological arguments have grown cold with the centuries. It is less a scholarly blessing that few disinterested historians have sifted the ashes to find what heat remains. Practitioners of church history are usually religious people aiming to excavate righteousness out of the past, and their work usually smacks of special pleading and presentism.

The Catholic historian Philip Hughes, whose three-volume work reigns over the field by its sheer bulk, saw the English schism as a great battle between king and pope with all the good Catholics upholding papal sovereignty over the church. He read his sources through the flawed lens of the First Vatican Council, and his work has the curious distinction of being infinitely better at interpreting the English Protestants than at understanding the English Catholics.

The publication of Jean-Pierre Moreau's book is thus an event of considerable significance, for his work is the best theological survey of Catholic thought in the English Reformation. "Who is Catholic?" This is the primary question he poses in his introduction, and through the rest of his book he shows us how complicated the answer to that question was in the first half of the sixteenth century.

Above all, Moreau demonstrates the ambivalence that many devout English Catholics had toward the papacy. Was the papacy of divine or human origin? Could it be supplanted? He points out that, even during the reign of Mary Tudor, Stephen Gardiner never claimed that the papacy was ordained by God but seemed to hold that papal power was a matter of

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statute. I am pleased that Moreau has accepted in large measure my reading of Thomas More's ambivalence about the place of the pope in the church. But, then, Moreau is one of the few scholars who have read much in More's own huge corpus of religious writing.

The papacy was not the only source of ambivalence. What part did kings play in the divine order of things? To what extent was monarchy a divine institution? In his discussion of Reginald Pole's theology, Moreau touches on the ambivalence that Catholics felt toward kings, who were, as Pole said, allowed by God rather than ordained as the best of all possible governors. Thomas More would have agreed.

Moreau's greatest virtue is the clarity with which he discusses the great question of the English Reformation, the question centuries old by the time of Henry VIII. "The stakes went far beyond the matrimonial life of the king and even the authority of the pope. It was a question of maintaining *the* religion and a society where one faith might reign" (p. 217). Nearly everyone assumed that division of the faith meant destruction of the land. But how could secular society and religious faith exist together? One did not have to be an extreme papalist like John Fisher to see that the meddling of kings in sacred affairs and the destruction of the ecclesiastical liberties endangered the fundamental Catholic doctrine of how God revealed himself to humankind. Thomas More saw as much and died as a martyr not for the papacy but for an independent church. Many shared his views if not his resolute courage.

Throughout his book Moreau probes at his sources with the delicacy of a paleontologist brushing at fragile fossils, laying bare the subtle differences in the thought of men of similar temperaments. A certain tragic sense hangs over these pages. Conservatives like Gardiner followed the king because they thought the alternative was national dissolution. They believed that they could preserve the essence of Catholicism in a national church. By the time they realized their error, it was too late to turn England around except by blood and fire. The compromisers became coercers under Mary Tudor and, under Elizabeth, were themselves coerced. Moreau tells the story with sympathy, power, grace, and, above all, with understanding.

RICHARD MARIUS
Harvard University

ROY E. SCHREIBER. *The First Carlisle: Sir James Hay, First Earl of Carlisle as Courtier, Diplomat, and Entrepreneur, 1580–1636*. (Transactions of the American Philosophical Society, number 74, part 7.) Philadelphia: The Society. 1984. Pp. 202. $20.00.

This short, workmanlike monograph tells the story of a figure of the second rank at the courts of James I and Charles I. James Hay was a Scot, of a poor cadet branch of a great Scottish family. A man of great charm and tact, he was also witty and a good conversationalist, though not handsome—the queen of Bohemia affectionately called him "camelface." So, although King James obviously enjoyed his company, Hay never became a favorite as Robert Carr and George Villiers were to be. Hay followed James to England in 1603 and made a place for himself at court, but he did not become one of the gang of courtier Scots, such as John Murray and Thomas Erskine, who surrounded James and served as conduits between the court and James's ancient kingdom. Hay instead did his best to turn himself into an Englishman. Both of his wives were English, and both of their fathers were reluctant to have their daughters marry a man they regarded as an upstart adventurer.

From the beginning the king used him on diplomatic missions; after 1603 Hay was the only Scot James employed systematically as a diplomat. Until 1619 his assignments were complimentary in nature as well as very showy and expensive. Between missions, Hay appeared regularly in the court masques, occasionally produced one himself, and also created for himself "a unique and virtually unassailable position as a banquet master" (p. 11). He became Master of the Robes in 1605 and Master of the Wardrobe in 1613; his conduct of both offices was notoriously extravagant and unbusinesslike. It is a commentary on James's lack of financial sense, his greatest failing as a king, that he promoted Hay to the wardrobe after eight years' experience of his conduct in the office of the robes. Hay was equally unbusinesslike in the conduct of his personal finances. Roy E. Schreiber gives an interesting account of Hay's various attempts to make money by exploiting his position at court. He tried his hand at many things: export licenses, concealed lands, fen drainage, wine imposts and licenses in Ireland, and various promotions in the West Indies, especially Barbados. None of these worked very well, and he ended his life heavily in debt.

Schreiber devotes most of his book to Hay's diplomatic career, from his first substantive mission, the mediation of the Bohemian quarrel in 1619, to the futile effort in 1628 to put together a military combination of smaller states that would help Charles and Buckingham to mitigate the consequences of their stupidity in trying to fight France and Spain simultaneously. Like most ambassadors, Hay was not in a position to make policy, and his suggestions on policy were often ignored. The author provides a careful explanation of the circumstances of each mission and how Hay handled them. His account is clear, sensible, and straightforward,

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but he nowhere gets very far below the surface. Furthermore, his style is rather uninspiring. I closed the book with a sense of disappointment: so much more might have been done with the life and times of a man whose diplomatic negotiations were by no means the most interesting aspect of his career.

MAURICE LEE, JR.
Rutgers University

MAURICE LEE, JR. *The Road to Revolution: Scotland under Charles I, 1625–37*. Urbana: University of Illinois Press. 1985. Pp. xvi, 258. $21.95.

With this volume Maurice Lee, Jr., provides a sequel to *Government by the Pen* (1980), his narrative history of Jacobean Scotland after the Union of Crowns. *The Road to Revolution* also completes a more or less continuous account of Scottish history from the time of James, earl of Moray, and the triumph of the Reformation to the outbreak of the Covenanting Revolution, a series of studies commenced more than thirty years ago. The only modern narrative of Scotland under Charles, its terminus was enjoined by David Stevenson's narration of the upheaval following 1637.

Readers will find Lee's picture of Charles familiar enough. He portrays the monarch as an appalling individual who was also disastrously incapable of handling his responsibilities. Moreover, the young king and most of his court were far more poorly informed of Scottish affairs than were Elizabeth and her ministers; he tellingly referred to his northern realm as "your country." Lee candidly admits that his story is a dreary one, and readers will probably find themselves looking forward to the far greater men who emerged with the revolution.

Nevertheless, there are some surprises. If Charles immediately dismantled his father's administrative structure and, as immedately, succeeded in antagonizing most of the aristocracy, he subsequently found an effective minister in William Graham, earl of Menteith. From 1628 to his fall in 1633, Menteith mitigated Charles's grosser stupidities and won aristocratic confidence to such an extent that, according to Lee, "something like . . . Jacobean normalcy returned" (p. 70). In addition, it is interesting to note the increasingly hostile Scottish attitude to the notion of Great Britain, an idea signally developed and promoted by Scotsmen in the first decade of the seventeenth century. Although Lee says virtually nothing about apocalypticism and its central place in Scottish political culture, he interestingly observes that Scotsmen discovered the numbers in Laud's name to add up to the beast's 666.

Lee's central interest is administration and the political machinations associated with it. Although he quite competently discusses underlying and often highly complicated social issues, they are tangential to his prime concern, and readers will need to go elsewhere for detailed and extensive analyses. Only in his final chapter does he undertake an assessment of the Caroline experience, and even here his main point is fundamentally about administration. In contrast to Stevenson and the "Whig" tradition following from S. R. Gardiner, Lee sees Charles's regime as a major departure from the sound practices of its predecessor. It would be Charles's new directions, policies, disruption, and mismanagement, rather than any underlying weaknesses, that ultimately destroyed the Stuart monarchy. Accordingly, he does not see a "general crisis" in the seventeenth century, and, like many writers, he finds the often-cited parallels between Scotland and Catalonia to be quite superficial. Lee is unsure whether Menteith could have prevented the revolution, but he has no doubt that Charles's misfortunes were entirely self-inflicted.

This book is unlikely to inspire new students to enter into Scottish studies, but, for the committed, it offers an extremely knowledgeable—if utterly one-dimensional—narration of politics before the revolution.

ARTHUR H. WILLIAMSON
New York University

RONALD HUTTON. *The Restoration: A Political and Religious History of England and Wales, 1658–1667*. New York: Clarendon Press of Oxford University Press. 1985. Pp. xiii, 379. $29.95.

Ronald Hutton's history of England from the death of Cromwell to the fall of Clarendon has many fine qualities. Most original of all is the attention it pays to the provinces and to the interplay of central and local interests. Hutton not only discusses the restoration of episcopacy but also describes an enthusiastic citizenry cheering the bishops into their cathedral cities. Likewise original is his portrayal of Richard Cromwell, whom he depicts as a man of charm and energy, not as an innocent and a weakling. Richard's one fault was to fail to tame the army, and this occurred because the junior officers deserted him. In fact, one of the most valuable insights in this book is the role of the junior officers: they brought down Richard in April and the Rump in October. Equally perceptive is Hutton's insistence that Monck did not bring about the Restoration by himself; his role was to pin down Lambert in the north while the Portsmouth garrison defected, the fleet mutinied, the City refused a loan, and troops in the southern two-thirds of the country submitted to Speaker Lenthall.

Hutton is as discerning in exploring Monck's motives as he is judicious in describing Monck's role.

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He finds that Monck's presbyterianism is the clue to his conduct, for presbyterianism led him to seek, through the preservation of an established church, to bring discipline and unity to the land. On other occasions Hutton looks with sympathy on ill-discipline: he finds, for example, the petitions and riots of the London apprentices to be the first boulder that started the avalanche toward the Restoration. This observation leads him to an ingenious theory that the Restoration was a reflection of a generation gap. Not only were the apprentices young but so too were Henry Cromwell, John Mordaunt, and the parliamentarians who determined the course of events in the Convention. Being a generation younger, they had known only the chaos of war, not the threat of royal tyranny. They had also known the aggressive conduct of the Quakers, whose role in the events of these years Hutton rightly emphasizes. The most useful part of Hutton's account of the final Restoration settlement is his detailed description of the enforcement of the Corporation, Conventicle, and Five-Mile acts. He does not make the mistake, which so many historians make, of assuming that because Parliament passed an act it was enforced.

Although Hutton's book contains these insights, it is for two reasons a disappointing book. First, it has no shape or architectural quality, no clear themes, and no developing argument, even within a chapter. Hutton is better at purveying information than he is at developing an argument. He has a keen critical intelligence (see his endnotes, which are in themselves a splendid critical bibliography) but not a synthetic intelligence, not the ability to generalize. Second, his conclusions are warped by his relentless desire to offer a revision of what earlier historians have written. He is too good a scholar to say that they are wrong where there is no evidence to prove it. So instead he dwells on qualifications, makes the incidental important, and overlooks the obvious (see especially pages 119 to 123 where he quotes Godfrey Davies's conclusions, purports to assess them, and then allows his own arguments to wander off into the sands. He seems afraid to say that Davies's conclusions were essentially right.

CLAYTON ROBERTS
Ohio State University

MARTINE WATSON BROWNLEY. *Clarendon and the Rhetoric of Historical Form*. Philadelphia: University of Pennsylvania Press. 1985. Pp. xv, 239. $24.95.

Marriages of history and literature are seldom made in heaven. Historians are apt to take style for granted, believing that style waits on the master. Literary figures are apt to treat historical facts as irksome, although sometimes useful for other purposes. Clarendon was exceptional in that he combined the methods of history and those of literature in *The History of the Rebellion*. He created a masterpiece of contemporary history that was equally a work of art. His originality appeared not only in the factual, historical narrative but also in the style, which made perceptions of character, irony, and accident serve the purposes of analysis. Despite its flaws the *History* remains a profound and moving work.

Martine Watson Brownley explains how Clarendon achieved his results. Her book is important not only because it analyzes Clarendon's methods and demonstrates how literature, religion, and political experience combined to make the historian but also because it provides a model of how literary criticism may be used to elucidate history and historical writing.

Historians have much to learn from this book about the political consequences of language and about how rhetorical form permeates the narrative and analysis. Brownley avoids fashionable literary jargon (although some esoteric rhetorical terms might have been defined), and she is emphatically concerned with the historical evidence for her arguments. She has used manuscripts, calendars, and Clarendon's letters with skill. The five chapter headings describe the book's contents. She emphasizes throughout that the *History* must be read as a whole, that style and understanding go together (p. 50), and that Clarendon's idealism and moral purpose did not prevent him from being a pragmatist in politics and a detached observer of other men's strengths and weaknesses. She argues that any attempt to study the character sketches apart from their context in the narrative distorts their purpose and leads to critical misunderstanding. The characters are not literary set pieces that simply imitate ancient models; they are integral parts of the larger history and, "like his best analyses of statecraft, Clarendon's characters are literary successes that evolved from practical political necessity" (p. 155).

Brownley recognizes Clarendon's faults, both as a writer and as a historian, but she accounts for them. Clarendon's legal training, his disputatious interest in theology, and his experience as a political controversialist all contributed, along with a sometimes priggish self-confidence, to his verbosity, syntactical involutions, and interminable sentences. "Significantly," she notes, "he is at his literary worst when he is poorest as a historian" (pp. 68–69). Although in no sense a biography, her book reveals much about Clarendon's character and his working habits.

Clarendon was at his best as a contemporary historian, writing about what he knew best. Ultimately, perhaps, he sacrified too much to politics. Brownley's book raises important questions and is a

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book to be reckoned with. Although not always a lark to read, it is always worth reading.

F. SMITH FUSSNER
EMERITUS
Reed College

GERARD REEDY. *The Bible and Reason: Anglicans and Scripture in Late Seventeenth-Century England.* Philadelphia: University of Pennsylvania Press. 1985. Pp. 184. $20.00.

Through the years I have tried unsuccessfully to interest graduate students in dissertations dealing with the many uses of the Bible in seventeenth-century England, when it was common coin in authority, from street parlance to high court politics. C. John Sommerville's *Popular Religion in Restoration England* (1977) treats religion at the lower social levels; for the middling sort, Margaret Jacob has given us *The Newtonians and the English Revolution* (1976); at the more rarified levels of theologico-political debate we have James Daly's *Sir Robert Filmer and English Political Thought* (1979), Robert Todd Carroll's excellent *The Common-Sense Philosophy of Religion of Bishop Edward Stillingfleet* (1975), and Frank E. Manuel's brilliant *The Religion of Isaac Newton* (1974).

Gerard Reedy bravely gives us the first expository survey of biblical usages for the last half of the seventeenth century. Although he says he relied on the ideas of four major Anglican divines—Isaac Barrow, Robert South, Edward Stillingfleet, and John Tillotson—his work is a virtual compendium of all major exegetic positions, from Anglican to Socinian. Reedy has something for everyone: Anglican responses to Hobbes, Spinoza, and Toland; the Bible as a guide to polity, with a perceptive section dealing with typology as it applied to Cromwell's role as a new King David; illuminating comments on Filmer's and Locke's positions on the Bible; and a central portion devoted to the debate over the canonical verifications and applicability of Scripture. Throughout, Reedy insists that Anglican apologetics remained loyal to pure biblical application to general moralities, always above mere political employments and not directly connected with overt rationalist and scientific justifications.

On this latter point, Reedy should provide stronger examples. Although he is certainly right in maintaining Anglicanism's centrist position on the Bible (his proofs for this are abundant), he seems to miss the Anglican pride in providing the nation's church with a particularly socially utilitarian theory. God gave his precepts, not ordinarily abstruse, to defend his social order, provide basic political sanctions, and show the way to the good life. All other (false) religions were abstractly authoritarian and socially impractical in their preachments. Anglican Christianity was sociable, practical in its piety, lawfully obedient to the state, and universally useful in its naturalism. This could seem to be mere accommodation to others, and Reedy rightly absolves Anglicans of this Vicar-of-Bray syndrome but without realizing Anglicanism's eminently social bases for biblical exegeses. God's Bible had to be an Erastian text and a primary guide for (English) social cohesion.

But these comments should not detract from the book's brilliance, thoroughness, and broad scope, which bridges religious, political, philosophical, and literary fields. One could wish only for a volume thrice its size for the author's attitudes on everyone's pet topics concerning the Bible's place in seventeenth-century England. Congratulations, too, to Paul Korshin for his noted encouragement of this germinal work.

GERALD M. STRAKA
University of Delaware

D. SZECHI. *Jacobitism and Tory Politics, 1710–14.* Edinburgh: John Donald; distributed by Humanities, Atlantic Highlands, N.J. 1984. Pp. ix, 220. $30.50.

D. Szechi's book demonstrates the persuasiveness of Geoffrey Holmes's *British Politics in the Age of Anne* (1967). Holmes argued that the age's politics was structured by contention between the Whig and Tory parties, and he distinguished the independent or "country" Tories as a major group in their party. Szechi's book is an investigation within the model of politics propounded by Holmes. It analyzes the Jacobite members of parliament as a segment of the country Tories, delineating the relation of the Jacobites to the country group and the effect of both on Britain's government during Harley's tenure of office.

The core of the book is an account, by parliamentary session, of the maneuvers of Jacobites and country Tories. Here again the book stands as a tribute to recent work on the politics of Anne's reign—especially to that of Holmes, J. H. Plumb, H. T. Dickinson, and P. W. J. Riley—for Szechi presents few bills and even fewer issues that have not already been identified as concerns of the country Tories. Szechi's book does fill some notable lacunae and allow more coherent depiction of the parliamentary scene. It provides a much fuller account of the activities of the country Tories than has heretofore been available and pioneers in integrating the history of the Scots in Anne's parliaments with that of the English. Szechi also illuminates the expectations and structures of the Pretender's court at St. Germain. That court both exerted remarkable

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influence over the parliamentary Jacobites and bemused itself with remarkably naive interpretations of English politics, thereby making it ready prey for an English politician eager to use the parliamentary Jacobites for his own ends. Szechi makes a convincing argument that, even when Harley and Bolingbroke negotiated with the Pretender, they did so as a means of securing the Hanoverian succession and their own dominance in parliament.

Szechi is unique in presenting later Stuart politics from the viewpoint of the country Tories. Readers may find this assessment from the back benches helpful for appreciation of the country Tories' political passions but inadequate as a basis for explanation of political events. Like the country Tories, Szechi characterizes Harley as a liar, and he attributes Harley's fall from power in 1714 to his mendacity—not a characteristic notably absent in more successful eighteenth-century politicians. Instead, as Szechi also demonstrates, assessments of the Tories' situation should emphasize the fissiparous nature of their party. Szechi argues that the Jacobites, as the extreme wing of the Tory party, were propelling their party toward instituting the one-party government later established by Walpole. Nonetheless, Szechi's description of internecine Tory strife about fundamental issues shows why many historians contend that the Tory party disintegrated shortly after the accession of George I. Perhaps the moral of Szechi's tale is that, in pudding time, those who fight about the pudding's recipe are handicapped in the contest for shares in its consumption.

NORMA LANDAU
University of California, Davis

DAVID MACKAY. *In the Wake of Cook: Exploration, Science, and Empire, 1780–1801*. New York: St. Martin's. 1985. Pp. 216. $27.50.

The title of this volume suggested to me a narrative of British exploits in the Pacific during the last two decades of the eighteenth century. It is, in fact, both more and less than that—geographically more since David Mackay is concerned with British activities worldwide, less because his chief focus is not the travails of explorers and scientists in remote colonies but rather the politics of government planning in London that preceded the voyages.

Mackay's organization is geographical rather than chronological—a decision that inevitably causes the reader some frustration when trying to conceive the possible relations of contemporaneous events. His introduction presents a competent survey of the political and scientific milieu of post-Cook England, stressing the empirical and utilitarian nature of British science in the early Industrial Revolution and the preeminence of Sir Joseph Banks in advising the government in all matters pertaining to exploration. If a single figure dominates Mackay's story, it is clearly Banks, heir to Cook as the voice of exploratorial experience and patron of innumerable projects and individuals abroad. Banks is also regarded as the principal purveyor of what Mackay repeatedly calls the "Cook model," although we are never given an explicit definition of this term.

The succeeding six chapters examine British imperial efforts in the southern Atlantic and West Africa, the Pacific coast of North America, the West Indies, and India. British interests in the southern Atlantic, as part of the sea link with India and the Pacific, intensified in 1780 when Dutch relations with France threatened British access to the Cape of Good Hope. The Pitt ministry also entertained plans of establishing a penal settlement on the West African coast. At the end of the decade, interest shifted to the whaling industry's need for ports of replenishment in the southern oceans.

In the North Pacific the fur trade, the continued search for a northwest passage, and the pursuit of territorial claims were the focuses of imperial efforts. Here, as elsewhere, Mackay gives as much attention to the expeditions that were planned, outfitted, but abandoned before setting sail as to those that met their goals.

The author wisely avoids retelling the saga of Bligh and the *Bounty*, choosing instead to describe the economic motivations of the voyage—"food for slaves"—and the botanical interests of Banks and others who organized it. Economics and botany also dominate the final chapters, which examine the attempts to acquire high-quality Indian cotton seeds for replanting in the West Indies and the search for raw materials whose production would bolster the Indian economy without threatening the industrializing mother country.

The book is firmly anchored in primary sources from the Admiralty, Board of Trade, and numerous other public and private collections. By detailing the failures as well as the fruitful plans, Mackay demonstrates that British imperial policy following the American Revolution was more chaotic and haphazard than has been generally realized. Historians of exploration will value this "view from the metropolis" as a supplement to the traditional voyage accounts. They may be disappointed, however, by the brevity of Mackay's treatment of the personalities of those involved and the expeditions themselves. The colorful account of Anton Hove's "commercial espionage" to discover exportable Indian cotton seeds is a welcome relief in this regard. As John Aubrey wrote of Isaac Newton's *Principia*, "I wish he had afforded a little more paper."

PHILIP F. REHBOCK
University of Hawaii

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F. P. LOCK. *Burke's Reflections on the Revolution in France*. (Unwin Critical Library.) Boston: George Allen and Unwin. 1985. Pp. 228. $25.00.

F. P. Lock's study of Burke's *Reflections* is the latest in the useful Unwin Critical Library series, edited by Claude Rawson. The series seeks to probe the background, to provide a critical interpretation, and to elucidate both the immediate reception and the subsequent discussion of familiar texts ranging from Charles Dickens's *Martin Chuzzlewit* to Ezra Pound's *Cantos*. As the general editor's preface makes clear, the volumes are intended for "serious students and teachers of literature, and for knowledgeable nonacademic readers." Lock has done this project proud. And, in fact, to any instructor who has puzzled over how best to lead undergraduates through the *Reflections*, that sometimes unfamiliar labyrinth, this text is a godsend.

Lock contributes a reasonably challenging interpretation of Burke's political career in general as well as of the *Reflections* in particular. His Burke is a consistent man of principle, albeit closer to J. R. Dinwiddy's utilitarian statesman than to Peter J. Stanlis's natural law philosopher, but nonetheless far removed from Sir Lewis Namier's time-serving politician. Lock rejects all critiques of Burke that seek to explain his supposed political and philosophical inconsistencies by distinguishing between a young liberal and an old reactionary statesman. Lock maintains that between the Burke of the 1760s and that of the 1790s there was an essential continuity of conservative thought and principle. He also argues that, if Burke had only entered politics during the long Pelhamite ascendancy rather than in the chaotic 1760s, with their weird contortions of issues and personalities, this would have long since been apparent to all, for Burke defended the same aristocratic property-holding elite throughout his life. His alleged liberalism in regards to the American Revolution, Ireland, and India was deceptive. In both America and Ireland Burke wanted reform chiefly to defuse more radical demands; in India, as in France in the 1790s, he was merely defending hierarchical, religious, and conservative society from rapacious despoliation.

Lock views the *Reflections* as resting primarily on historical argumentation rather than on that philosophical controversy that Burke mistrusted and even feared. Written for an immediate audience and not for the ages, the tract is a "work of persuasion not of philosophy" (p. 90) and, hence, resists comparison with any ordered set of ideas or beliefs such as Filmer's *Patriarcha*, Hobbes's *Leviathan*, and Locke's *Two Treatises*. Indeed, Lock finds the *Reflections*, with its espousal of definite social values coupled with this lack of philosophical system, closer to *Gulliver's Travels* than to *Leviathan*. This should spark an extended discussion.

The two chapters on the contemporary and subsequent critical history of the *Reflections* are generally insightful. Lock believes that he has found, in the statesman's increasing distrust of Dissenters, one legitimate dichotomy between the young Burke and the old Burke. With that in mind, it is instructive to read how, in 1790 and 1791, the text was widely viewed as an attack on further religious toleration. Lock thinks that in the period after his death "Burke's reputation gradually climbed," with the *Reflections* seen as "an almost inspired masterpiece" (p. 172) and his other later writings viewed "with the veneration of sacred texts" (p. 167). Some at the time disagreed. In the 1820s, for example, J. W. Croker in the *Quarterly* thought Burke's name regrettably "obscured"; *Blackwood's* correspondent remarked that Burke's "ashes ... slumber almost without notice"; and, from within the Hazlitt circle, James Northcote found Burke "little talked of."

JAMES J. SACK
University of Illinois, Chicago

SONIA LIPMAN and V. D. LIPMAN, editors. *The Century of Moses Montefiore*. (Littman Library of Jewish Civilization.) New York: Oxford University Press, for The Library and the Jewish Historical Society of England. 1985. Pp. x, 385. $27.95.

A hospital in the Bronx, a club in Montreal, synagogues in various parts of North America—these testify to the legendary attributes associated with the name of Moses Montefiore, an outstanding English Jew of the nineteenth century. Born in 1784, he quickly amassed a fortune but devoted the greater part of his life not to augmenting it but to using it for philantropy; he rose in Gentile society but remained strictly observant in the Jewish law. Having paid five visits to Palestine, the first as early as 1827, he can be regarded as a proto-Zionist; in 1840, imminent danger to the Jews of Syria drew him to Damascus, and in 1846 the harsh anti-Semitic policies of Tsar Nicolas called him to Russia. That the Jewish Historical Society of England should commemorate the centenary of this patriarch is only proper, and the society is to be congratulated on the wise choice of Sonia Lipman and V. D. Lipman as the editors for the present volume.

Sonia Lipman opens the work with a general review of Montefiore's life, "The Making of a Victorian Gentleman." Thirteen essays by specialists on various aspects of his many-sided career follow. The text suggests careful editing, and the work is enhanced by an ample genealogical table, a chronol-

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ogy of Montefiore's life, and ten pages of illustrations.

In several places the hagiology that for a century has surrounded Montefiore is tactfully dispelled, and the record of his public career is well rounded. Its counterpart, his private life, remains in the shadows. Presumably, the material was not available: Montefiore's diaries and private papers were destroyed after his death. In 1862 his wife of fifty years passed away. Her memory continued to inspire him, and it would be useful to establish how far her piety shaped his career, transforming a Regency financier into an exemplar of Victorian philantropy.

But how far did the advocate of freedom and tolerance outside the camp approach these virtues within it? Montefiore's hostility to the infant Reform movement in Judaism is alluded to in this work but seems to be kept in the background, especially considering its significance for the development of the Jewish community of England. In 1842, on returning from his mission to alleviate the sufferings of the Jews in Damascus, Montefiore was the principal mover in launching an unparalleled decree of excommunication against a small, moderate Reform congregation that had just opened in London, and he maintained the barrier against Reform Judaism to the end of his days. This episode was worthy of closer inquiry.

S. D. TEMKIN
State University of New York, Albany

HEW STRACHAN. *Wellington's Legacy: The Reform of the British Army, 1830–54.* Dover, N.H.: Manchester University Press. 1984. Pp. viii, 302. $40.00.

Hew Strachan has written an excellent study of reforming efforts in the British army during the two decades before the Crimean War. In so doing, he reexamines the organization and structure of the pre-Crimean British army and takes issue with the often-cited thesis that little had changed since the Napoleonic wars and that military reform in Britain dates from the embarrassment and public outcry following the Sevastopol campaign. His central theme is that, in spite of conservative and frequently unsympathetic military and political leadership, often centered on the aging duke of Wellington, the British army was making significant progress in reforming itself in many areas on the eve of the Crimean War. The year 1854 found the work incomplete, and the failure to reform the higher administration of the army tended to undermine the progress that had been achieved in other areas.

Focusing his study on the organization and structure of the army, Strachan provides detailed information about the objectives and goals of the reformers and then concentrates three chapters on the life, service, and education of soldiers and their officers. He concludes that positive changes were being effected especially in the areas of length of service, health care, and education. A rising professionalism among the officers, reenforced by stronger educational requirements, characterized the period. The establishment of camps for field training and large-scale exercises became realities in spite of political opposition. Unfortunately, attempts to establish a legitimate General Staff for the army were less successful.

The nature and scope of the extensive British military commitment often complicated and exacerbated reform attempts. Forces were necessary for home defense as well as for policing and protecting the ever-expanding empire. Matching manpower with mission, developing proper training, providing appropriate equipment and the necessary supply line for troops stationed in so many parts of the world severely taxed the British military establishment. Political and economic considerations frequently either hampered or prevented reform efforts. By 1854, however, limited internal reform enabled the British army to meet both its home and its imperial responsibilities. Unfortunately, it was ill prepared for an additional theater of operations on the European continent. The results, Strachan concludes, might have been worse had it not been for the two decades of reform preceding the Crimean War.

Wellington's Legacy is a tightly organized and well-written book. Strachan's impressive research lends support to his thoroughly developed conclusions. This work contributes significantly to our understanding of the organization and structure of the pre-Crimean British army.

GORDON C. BOND
Auburn University

ROYDEN HARRISON and JONATHAN ZEITLIN, editors. *Divisions of Labour: Skilled Workers and Technological Change in Nineteenth-Century England.* Urbana: University of Illinois Press or Harvester, Brighton, England. 1985. Pp. ix, 254. $29.95.

This book displays several of the characteristics that one associates with a collection of essays by various authors: it lacks a clear sense of editorial direction and control. Although the majority of contributors are students or former students of the Centre for the Study of Social History at the University of Warwick, Royden Harrison's assertion that "the editors became more *dirigiste* than either tradition or their own inclinations warranted" seems to have little basis (p. viii). Neither the editors nor the authors make clear why they decided to direct

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attention toward cabinetmakers, bakers, sailmakers, potters, shipbuilders, engineers, and compositors, especially since the studies of the first two groups are rather pedestrian. Nor is it easy to reconcile Harrison's insistence on the rentention of the concept of the labor aristocracy with the evidence produced by Mark Hirsch for sailmakers, by Keither McClelland and Alastair Reid for shipbuilders, and by Richard Whipp for North Staffordshire potters. Indeed, as Whipp remarks, "No group of pottery workers fulfilled even a majority of the features of the labour aristocrat as designed by Hobsbawm" (p. 42).

Nonetheless, the book makes a significant contribution to our understanding of skilled labor in the Victorian and Edwardian economy. Its subject matter—"the nature of craft regulations . . . the sources from which they arose and . . . the circumstances which determined whether they were maintained in the face of attempts by employers to introduce de-skilling innovations or otherwise redefine the existing divisions of labour" (p. 7)—is of fundamental importance and the source of continuing debate. The contributors demonstrate conclusively that the introduction of technological and other innovations did not necessarily reduce the demand for skilled labor and so diminish the bargaining position of the skilled worker. The relationship between industrial development and craft regulation was much more complex than that. As Jonathan Zeitlin explains in the fine comparative study of engineers and compositors that brings the volume to a close, "the alliances and cleavages within the ranks of capital and labour alike which exerted so strong an influence on the outcome of industrial conflict and the future of craft regulation can best be understood as the product of a dialectic between such structural factors as market position and the shape of the existing division of labour, and the creative response of each group of actors to the dangers and opportunities presented by any historical situation" (p. 241).

Like so many collections of its type, *Divisions of Labour* leaves the reader both pleased and irritated: grateful to have available the results of important recent research but disappointed that the issues raised are neither addressed more directly nor explored more consistently.

JOHN BENSON
Wolverhampton Polytechnic

DAVID J. V. JONES. *The Last Rising: The Newport Insurrection of 1839*. New York: Clarendon Press of Oxford University Press. 1985. Pp. 273. $21.95.

The night of November 3–4, 1839, in Newport, Monmouthshire, saw the last occasion on which a substantial force of armed citizens challenged the state in mainland Britain. Faced with this crowd of thousands of men, British soldiers and policemen fired on them and put them to flight. The leaders of the crowd were taken, and the three most prominent were sentenced to the ancient barbaric punishment of death by hanging, drawing, and quartering. These leaders were not poverty-stricken wretches but men of education and some local standing. John Frost had been the mayor and a justice of the peace in the borough of Newport, Zephenia Williams was a mine agent and innkeeper, and William Jones was a skilled artisan in good standing in his friendly society and other community institutions.

Events on a smaller scale than those at Newport have toppled thrones and governments, and it seems surprising that these have had to wait for a century and a half for their historian. Only three serious studies have been written on this period: the biography of John Frost published for the centenary of the events by the late David Williams, and two recent studies *South Wales and the Rising of 1839* by Ivor Wilkes (1984) and the present study by David J. V. Jones. Newport, however, does not fit in happily with the major trends in British labor historiography. Williams, though clearly writing with sympathy for Frost himself, concluded that the events of November 4 were not an attempted rising but a "monster demonstration" that went wrong. He could not concede that the essentially peace-loving, nonconformist Welshmen of the valleys could have countenanced the levying of arms against Her Majesty. For those historians who saw the Chartists as potential revolutionaries misled by cowardly leaders, the events presented the greater problem of how such a working-class force could have been routed by a mere handful of soldiers. The events seemed likely to call into question a whole philosophy of history.

Williams made a close and scholarly examination of material in Welsh and English relating to the rising; his conclusions, however, sit uneasily on his own evidence. Perhaps it needed the renewed interest in social history to reach the conclusions that Jones does with great discernment and conviction.

The history of the Newport events has suffered from numerous sociological preconceptions. Chief among these was the idea that radical artisans and small businessmen in the Chartist period subscribed to a kind of Painite doctrine that laid great stress on constitutional methods to gain reform. Such men tended to dissenting religious affiliations and to a belief in universal suffrage and the reform of political structures in order to defeat exploitation. They have therefore been identified as supporters of moral force against physical force. But what this kind of analysis too often ignores is that the Painite tradition, which certainly provided one major source of political vocabulary for early nineteenth-

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century radicals, was based on the defense of two armed revolutions. The constitutional arguments included the justification of armed rising against oppressive and tyrannical regimes as an examination of the rhetoric of any major radical spokesman in the 1830s will show.

In this superb study of industrial and political responses among the miners and ironworkers of South Wales, here is no "monster demonstration" nor yet a "riot" in any recognizable sense of the word. This is a society torn by class conflict, in which the workers identified the source of their grievances in unjust laws and irresponsible authority and sought the solution in the Chartist program for forcing the admission of working men to the law-making process. Jones shows a face of Chartism that contemporaries and historians have avoided or rejected—the mixture of conviction, loyalty, and terrorization, by which plans were kept secret and adhesion ensured. These methods, together with the need for absolute secrecy, contributed to the rising's failure, just as they would have been essential elements in its success if things had gone differently. But this account presents a genuine popular movement, a movement of the excluded and the powerless as it must have been, warts and all.

DOROTHY THOMPSON
University of Birmingham

KEVIN L. MORRIS. *The Images of the Middle Ages in Romantic and Victorian Literature*. Dover, N.H.: Croom Helm. 1984. Pp. 259. $38.50.

This is a disappointing book. To begin with, the title is misleading, for the study has nothing to do with "images" and more to do with theology, history, architecture, and art criticism than with literature. There is virtually nothing on Keats, the Gothic novel, Bulwer Lytton, Charlotte Mary Yonge, Charles Reade, Tennyson, William Morris, the recovery of Arthurian materials, the century-long debate over the relative merits of the Saxon and the Norman inheritance, or the reinterpretation of Dante by the pre-Raphaelite painters and poets. Major writers are scanted—Carlyle gets two pages; Blake and Arnold receive a single paragraph each—and most of the literary analysis is heavily dependent on summary, quotations taken out of context, and secondary authorities.

What interests Kevin L. Morris is the revival of religious medievalism, which he interprets as a reaction to the emotional aridity of the Enlightenment and the spiritual impoverishment of the Established Church. He connects its resurgence to romanticism, since both were concerned with forms of the imagination not canonized by Lockean psychology, neoclassical aesthetics, or deism. But he distinguishes between the two on the grounds that "religious medievalism was imagination plus the desire for a communal language" (p. 223) of secular and spiritual authority, whereas romanticism, like Protestantism and rationalism, generated an individual syntax of self-expression and salvation.

Morris differs from Hoxie Neale Fairchild, whose six-volume study of *Religious Trends in English Poetry* (1939–68) is the standard authority in this domain, by taking religious medievalism more seriously; Morris believes it goes beyond mere antiquarianism and aestheticism to an expression of metaphysical yearning ultimately more orthodox, more historicist, and, in some ways, more European than English literary romanticism. On the distinctions among varieties of religious medievalism Morris is exacting and persuasive: he separates Romantics from religious revivalists, Roman Catholics from medievalists, Cisalpines from Ultramontanes (especially as regards their taste in ecclesiastical architecture, to which he devotes a chapter), and Anglo-Catholics from Tractarians; he also discriminates phases of advocacy and attack, from Thomas Warton and Kenelm Henry Digby to Charles Kingsley and John Ruskin.

Morris concludes, first, that religious medievalism because of its Catholic (orthodox, communal) quality cannot adequately be described as a Romantic phenomenon; second, "that literary religious medievalism was a serious, and ultimately metaphysical quest" (p. 230); and third, that the medieval church, though viewed from many vantage points in the eighteenth and nineteenth centuries, was widely perceived as once having cleansed and reinvigorated a decadent society and, thus, as offering hope for a world weary of rationalism, individualism, and materialism.

I do not quarrel with these conclusions; indeed, I have considerable respect for them because of the formidable reading Morris has brought to bear on his theses. But to reach these conclusions requires a hard slog through sometimes opaque, overly qualified prose: "Scott's apparently deliberate socially and psychologically self-protective ambiguity" (p. 134) is a mild example. Morris follows "the inductive principle," which he admits "leaves much to be desired" (p. 2). Most of all, it leaves him without benefit of many recent syncretic formulations of romanticism by such prominent synthesizers as Northrop Frye (whose notion of a "Romantic topocosm" would have been very helpful at several points in Morris's exposition), Harold Bloom, Geoffrey Hartman, Earl Wasserman, and Paul de Man. The one paradigm of literary romanticism that Morris employs, M. H. Abrams's "natural supernaturalism," seems to be understood shallowly and without reference to Abrams's earlier, equally indis-

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pensable paradigm articulated in *The Mirror and the Lamp* (1953).

The publisher, Croom Helm, has not helped Morris's case. No copyediting was done, so solecisms, errors in the typescript, inconsistencies in bibliography and footnotes, misspellings, wrong word divisions ("witne-ssed," "del-ight"), and unnecessary repetitions disfigure the typescript facsimile, which is wearisome to read because of the light inking in which its photographic reduction from single-spaced pages is printed.

ROBERT L. PATTEN
Rice University

CATHERINE GALLAGHER. *The Industrial Reformation of English Fiction: Social Discourse and Narrative Form, 1832–1867*. Chicago: University of Chicago Press. 1985. Pp. xv, 320. $25.00.

Interdisciplinary works frequently promise more than they deliver, but Catherine Gallagher has written a masterful study that should prove as useful and provocative to historians as to literary scholars. As a literary critic, her concern is to examine what she calls the "prehistory" of the Victorian canon, which is to ask why twentieth-century critics have declined to accept the early Victorian industrial novels as part of that canon. Simply put, her answer is that we have inherited Matthew Arnold's insistence on culture's separateness from and superiority to politics. But, in searching for the roots of this rejection of the social and political concerns of the earlier novelists, she proves herself to be a consummate intellectual historian. We are given a complex and fascinating account of the evolution of moral discourse in England from the debate over slavery to the "condition of England" question and its aftermath.

The various discourses are treated dichotomously, with the book organized into three sections: "Free Will versus Determinism," "Family versus Society," and "Facts versus Values." In less able hands this framework might have proved a straitjacket, but here it serves admirably to organize and interpret a great deal of disparate material and to reveal the contradictions and impasses in the literature. The author casts an unusually wide net, gathering in such major figures as Wilberforce, Owen, Martineau, Carlyle, Dickens, and George Eliot, plus a host of minor ones. She also examines the kinds of discourse to emerge within such political movements as the antislavery crusade, factory reform, Chartism, and the second reform act. These authors and movements are not employed simply to provide "historical background," à la Basil Willey, for Gallagher manages to achieve a truly integrative analysis of the major discursive elements of the period—social, political, and literary.

Although the book is certainly not an easy read, Gallagher's writing is both lucid and elegant, in spite of an occasional tendency to brandish "lit crit" jargon. There is an insistent if gentle denial of Marxist analysis here, even if one must often turn to the endnotes to find criticisms of writers such as Terry Eagleton. The same could be said of other areas where established views are challenged or controversial interpretations, such as that of Martin Wiener, are supported. This is not a criticism of the author who, given the density and complexity of her analysis, wisely chose not to render the text unwieldy by interweaving detailed historiographic discussions. It is an injunction to the reader not to skip the endnotes.

ANTHONY BRUNDAGE
California State Polytechnic University, Pomona

ROGER COOTER. *The Cultural Meaning of Popular Science: Phrenology and the Organization of Consent in Nineteenth-Century Britain*. (Cambridge History of Medicine.) New York: Cambridge University Press. 1985. Pp. xiii, 418. $37.50.

Roger Cooter's new book attempts to provide a case study of the social and cultural effects of science, especially the role and ramifications of science among "the common people," a topic he claims has been largely ignored by other historians. He chooses to focus attention on Franz Gall's craniology "because phrenology is poised between the intellectual boundaries that have come to be erected between science and pseudoscience, nature and culture, science and society" and thus "provides the incentive to expose and evaluate those boundaries" (p. 8). At the same time, and *pace* "the condescensions wrought by the entrepreneurs of the 'Darwin industry,'" he suggests that it was through the impact of phrenology in the early nineteenth century that laymen came to apply the perspectives of scientific naturalism to all manner of social, moral, and political questions and to see reality through this particular ideological lens. For Cooter, then, the study of phrenology provides an instrument through which to examine the sorts of normative assumptions embodied in science, an exemplification of his claim that science somehow constitutes "a mystified mediation of capitalist social relations" (p. 205) and affords valuable insights into "how and why some conceptions of reality acquire the mantle of objective scientific truth while others come to be regarded as arrant nonsense" (p. 35).

Following a brief discussion of the historiography of phrenology and the reasons for its peculiarly

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potent appeal to members of the medical profession in early nineteenth-century England, Cooter turns to a broader examination of the social roots of support for, or opposition to, Gall's doctrines. Using the techniques of prosopography, he provides persuasive evidence of linkages between people's social location and experiences and their allegiance to phrenology, suggesting that this lends considerable weight to his attempt to provide a social interpretation of the debates over its scientific status. More specifically, he points out the close connections between an embrace of the new "natural knowledge," the ambiguous social status of marginal professionals, and the attractions for such a group (the "thinking class") of a doctrine that provided a physiological account of the "natural" predominance of the intellectual and moral faculties.

Subsequent chapters examine the role of George Combe in the popularization of phrenological doctrines; the rise of ordinary people's enthusiasm for the subject; the growth of phrenological study groups; the two hundred itinerant lecturers who preached in behalf of the new knowledge; and the associated transformation of phrenology into a conservative instrument that induced "consent to bourgeois liberal rule" (p. 165). Finally, he attempts to provide a solution to what must be seen in this context as a paradox, the cultivation of phrenology and phrenomesmerism by even the most radical segments of the working class.

Based on wide-ranging research in both published and unpublished materials on phrenology, Cooter's book does manage to deepen our understanding of its social and symbolic significance. At the same time, his often heavy-handed theorizing, turgid prose, and sprawling presentation of his arguments make his monograph anything but a pleasure to read.

ANDREW SCULL
University of California, San Diego

BRUCE L. KINZER, editor. *The Gladstonian Turn of Mind: Essays Presented to J. B. Conacher*. Buffalo: University of Toronto Press. 1985. Pp. xv, 294. $35.00.

The idea of Gladstone as a theme for a tribute to James Conacher was a good one. Conacher himself made his wider reputation as a scholar of the Peelites, but his contribution to Gladstone scholarship neatly fits into that transitional category. A practitioner of what has since become defined historiographically as "high politics," he distanced himself decisively from what this book sums up as the "old orthodoxy" of John Morley and J. L. Hammond. Yet, as Trevor Lloyd points out in his thoughtful overview of the collection, Conacher shares little of the critical predispositions about politicians and power hunting that seem to be contributing toward the making of a new orthodoxy. Like good Lord Aberdeen himself, Conacher takes the view that, in the absence of evidence to the contrary, what people say can be believed. The great value of this book is precisely the extent to which it allows us to put that kind of predisposition to the test.

The two initial contributions deal with Gladstone's attempt to serve the church in the councils of the state. In "Conscience and Politics: Gladstone's First Book," Richard Helmstadter offers the fullest and most comprehensive study we have on *The State in Its Relations with the Church* (1838). He makes excellent use of the published *Diaries* and has the benefit of Perry Butler's *Gladstone: Church, State, and Tractarianism* (1982). His interpretation is complex and always interesting, although it tends to predate Bishop Butler's commanding influence. John Kenyon's piece, "Gladstone and the Anglican High Churchmen," traces Gladstone's growing ecclesiastical flexibility and the problems this created for him politically. Both Helmstadter and Kenyon deal more tenderly with Gladstone's problems of conscience than others have done of late (I get something of a rap on this account), but the new critical edge is detectable.

Then come two valuable and revealing pieces on Gladstone and women. The first is in the public dimension. In "Bird's Eye View of Gladstone," Ann Robson demolishes convincingly the myth that Gladstone "betrayed" the female suffrage movement that the Liberal party would otherwise have seen to completion. Gladstone's genius for filigreed evasion needed rather more acute interpreters than the feminist movement then supplied. In "Mr. Gladstone's Daughters: The Domestic Price of Victorian Politics," Patricia Jalland deals with the private dimension, pointing out with uncompromising directness the extent to which the self-centered routine of the "grand old people" at Hawarden imposed what would now be regarded as intolerable burdens on their daughters.

Three contributions deal with what might be called mainstream political themes. Peter Marsh's "Tearing the Bonds: Chamberlain's Separation from the Gladstonian Liberals" offers us some of the first fruits of his work on Chamberlain. With unrivaled expertise he draws attention to the extent to which Gladstone ruthlessly exploited his incontestable political ascendancy. Gladstone had no great difficulty in making Chamberlain the victim; the central point of interest is that the prime minister also chose to make Chamberlain the villain. Sue Brown, agricultural attache at the British Embassy in Washington—the catchment of contributors here

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is vast—points out in "One Last Campaign from the G.O.M.: Gladstone and the House of Lords in 1894" that this episode by no means deserves the casual neglect to which it has been consigned. And K. A. P. Sandisford reviews "Gladstone and Europe" in a way that brings out most valuably the conclusion that the "conservative element in Gladstone's foreign policy has been underestimated by most historians" (p. 189).

Finally, Deryck Schreuder, in "The Making of Mr. Gladstone's Posthumous Career: The Role of Morley and Knaplund as 'Monumental Masons,' 1903–27," looks with a sympathetically critical eye at the construction of the "original orthodoxy" of Gladstone's interpretation. He concludes that, although this orthodoxy is now at a discount, no new orthodoxy has emerged as yet. If a "new orthodoxy" does eventually emerge, this collection will have played an important role in helping to bring it about.

RICHARD SHANNON
University College of Swansea

KEITH M. WILSON. *The Policy of the Entente: Essays in the Determinants of British Foreign Policy, 1904–1914.* New York: Cambridge University Press. 1985. Pp. viii, 199. $34.50.

Heralded as a critique of the "orthodox" interpretation of British foreign policy from 1904 to 1914, this collection of essays by Keith M. Wilson seeks to identify the "actual content and determinants" of the policy of the Entente (p. 2). The result is a careful dissection of entente policy, especially its ambiguities and the public non sequiturs offered to explain relations with France and Russia. Wilson's arguments can be summarized quickly: the policy of the Entente rested on contradictory principles; imperial concerns, not European, always prevailed in Whitehall debates; Russia, not Germany, represented the true enemy; and cabinet politics shaped the evolution of entente policy. The Anglo-German naval race served as a convenient excuse for the Foreign Office; Wilson insists that Whitehall used Germany to mask the real intent of entente policy: defending India against Russia. But this assessment of the naval race remains unconvincing. Certainly, pre-1914 decision makers and subsequent historians (most recently Paul Kennedy) would not agree. On Admiral Sir John Fisher, naval negotiations, and parliamentary debates about naval power, Wilson is strangely silent.

He also rejects the idea of continuity in British foreign policy from 1904 to 1914. Focusing on the bitter clashes between Sir Edward Grey and the radical members of the cabinet, the author insists that Grey never possessed full backing for his foreign policy. Hence, Wilson argues that continuity in British foreign policy did not exist. Only Grey's repeated threats to resign, the shift in Liberal political fortunes after 1910, and constant "dissimulation," not a consensus among the British political elite, kept entente policy intact.

Two other axioms—British pursuit of a Continental balance of power and possession of a "free hand"—are also attacked. Without a large army Britain could not, Wilson declares, influence the European balance of power; thus, entente policy was aimed at protecting imperial concerns, not maintaining a Continental balance of power. Wilson notes, however, that Grey devoted himself so stridently to the Entente that he converted the imperial agreements into a virtual alliance commitment for Continental intervention. Thus, the author concludes, Britain lost its "free hand." In this commitment the military conversations played a key role, but he asserts, as if it were an original idea, that the talks did not compel British intervention. No recent historian has, however, made such an argument.

No more convincing is his analysis of the British decision to intervene in August 1914. Wilson attributes that act to Tory pressures, Grey's threats of resignation, and the overriding need to preserve cabinet unity to keep the Liberal party in power. Germany's violation of Belgian neutrality helped in the decision but chiefly to ratify it. Although historians might differ about which of these elements to emphasize (and most would give more attention to the moods of David Lloyd George), Wilson's effort at originality falls short. No "orthodox" historian would dispute much of his analysis of the British decision to go to war.

The essays, finally, pose a more troubling methodological problem. Their insistent focus on ideas and words suggests that foreign policy takes place only in the minds of foreign secretaries and Foreign Office officials. Wilson concedes the influence of the larger strategic environment, but only as a corollary to his arguments about entente and imperial policies. Public opinion, Parliament, and, most important, *Aussenpolitik* factors play almost no part in his assessments. British foreign policy is discussed in "Little Englander" terms, with plenty of criticism for the policy makers and hints of alternate policies—though none are explicitly suggested. An intellectual, noncontextual, almost deconstructionist approach to diplomatic history offers little long-term promise if not crafted more thoroughly. That is regrettable, for the author possesses the capacity for brilliant insights into complex issues. The problem is, however, his failure to acknowledge a changing external context that constantly reshaped the agenda of policy makers as they addressed day-to-

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day issues. Balance, as well as insights, make for better analysis.

SAMUEL R. WILLIAMSON, JR.
University of North Carolina

IAN F. W. BECKETT and KEITH SIMPSON, editors. *A Nation in Arms: A Social Study of the British Army in the First World War*. Dover, N.H.: Manchester University Press. 1985. Pp. x, 276. $32.50.

In 1914, Britain was, as E. M. Spiers points out in a discussion of the regular army in that year, "anything but a nation in arms" (p. 57). By November 1918, however, the British nation had achieved that exalted status to which prewar advocates of universal military service had called it. This "nation in arms" was manifested not only in the passing of 5.7 million persons through the ranks (720,000 died there) but also in the millions of under- and overage males who served in the volunteers and special constabulary as well as the involvement of women and children in various wartime voluntary or youth organizations. Although the participation of so much of the population in military or militarized activities had enormous social impact, it had curiously little long-range effect on the army itself. As Keith Jefferies notes in his essay on the postwar army, from the perspective of the 1920s it appeared that the "creation of a 'nation in arms' changed society more than it changed the army" (p. 231).

To deal with what John Keegan, in his brief foreword, calls "a neglected aspect" of the war—that is, the nature and composition of the army and its relationship to the rest of the nation—Ian F. W. Beckett and Keith Simpson have pulled together a useful and enlightening collection of essays. Students of tactics or strategy will have to look elsewhere, but anyone interested in the social as well as the military impact of the war will find something of interest here. All eight essays are well researched (although all of the many references to the Royal Welch fusiliers are misspelled (see Robert Graves's *Good-bye to All That* [1929], pp. 85–86) and competently written (although some at intervals may become a bit tedious or obvious). For example, it seems unnecessary for Simpson to remind us that the "overwhelming proportion of officer casualities occurred on the western front" (p. 80). But he more than redeems himself in his study of the officer corps by dispelling the myth of the inexorable fate of the "three-week subaltern" in illustrating that on average a new officer could expect to serve six months before becoming a casualty (cold comfort) and by pointing out that, although the war did considerably reduce the "'gentlemantly' intake into the officer class" (p. 91), it had little long-range effect on "the ethos and perceptions" of that class (p. 92).

Inevitably in such a collection, readers will find some chapters more perceptive and enjoyable than others. My own favorites are Beckett's broad but remarkably inclusive introduction to the general theme, Peter Simpkins's pioneering and evocative discussion of the hitherto-ignored problem of billeting massive new armies on the civilian populace, and, especially, J. M. Winter's fascinating chapter on how the war influenced class assumptions in the most class-ridden society in Europe. The war's impact, Winter writes, was at least as great on civilians as on those who served; furthermore, for the working class that impact was mainly positive. Using statisical tables to illustrate, among other things, a marked decline in infant mortality during the war, Winter concludes that wartime demographic gains for Britain outweighed losses and that, paradoxically, "because of armed conflict, the country became a healthier place to live in" (p. 207).

One other aspect of this volume that deserves mention is Simpson's concluding annotated bibliography. No doubt it would be impossible to assemble a bibliography of three hundred pages, let alone one of thirty, that would escape criticism for its exclusions. Why, for example, does he cite D. French's work *British Economic and Strategic Planning* (1982) as instructive on munitions while ignoring R. J. Q. Adams's *Arms and the Wizard* (1978), which is vastly more informative on that topic? But, taken altogether, Simpson's listing is an extremely helpful and comprehensive guide. Even if the bibliography is not by itself worth the price of the book, it is worthy of high praise.

THOMAS C. KENNEDY
University of Arkansas

K. D. BROWN, editor. *The First Labour Party, 1906–1914*. Dover, N.H.: Croom Helm. 1985. Pp. 297. $31.00.

This is a valuable collection containing the latest research on the history of what K. D. Brown chooses to call the "first" Labour party, from the general election of 1906 until the outbreak of the First World War. The twelve essays and Brown's introduction tend to be party history in a gentle, indeed almost hushed, tone that emphasizes how the Labour party of the time was "the radical wing of the Liberal party" (p. 260) and, although called a party, was really "an outsize, provincial, pressure group" (p. 268). The first six essays, written by various hands, dwell on the internal history of the party, in localities such as Wales and Scotland and in municipalities. These essays also explore the party's attempts to develop its institutional affiliations, such as the creation of a press, which led to the unfortu-

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nate rivalry between the *Daily Citizen* and the *Daily Herald*. It was an emblematic fight, not unknown in today's Labour party. In this case, the *Herald*, attempting to be something different and not merely to ape existing institutions, survived. The dominant issue during these years was how different the Labour party should or could be from the radical wing of the Liberal party.

This question was particularly prevalent in election history. A secret electoral pact between Ramsey MacDonald and Herbert Gladstone worked to minimize fights in English constituencies between Labour and Liberal candidates during the election of 1906 and the two elections of 1910, resulting in forty-two Labour members of Parliament. It is largely hindsight, reinforced by Arthur Balfour, that makes this agreement so significant for the decline of the Liberals and the rise of Labour. In the Labour party as presented in most of these essays, practical questions—such as how to cope with the economic dilemmas of workers—are much more important than theoretical problems. The party preferred higher wages to government action, although the latter was also pursued, most notably in the support of free meals for school children. Labour MPs continually fought slurs on the character of the working class, pointing out, with justification, that fondness for drink and idleness were as likely to be found among members of Parliament as among workers. Two essays deal with the uneasy relations between the Labour party and those who might be considered their "natural" allies, women (by Martin Pugh) and the Irish (by Dan McDermott). These groups, as well as the Liberals and the constituent parts of Labour itself (such as the extremely powerful trade unions), attempted to pursue their own special interests. The essays dealing with them are very much in the tradition of British laborism, intensely practical; Marx and the contradictions of capitalism are only mentioned in a dismissive way.

The ironies of the story are suggested in the last essay by A. J. A. Morris on Labour and foreign affairs, an area in which little interest had been expressed, other than the mouthing of Liberal platitudes; the idea of a general strike to prevent war was rejected by the vast majority of the movement. Yet, as Morris points out, by opposing the war in the old Liberal tradition, the minority of the Labour party—most notably, Keir Hardie and MacDonald—helped establish a distinctive voice for Labour that would be crucial in its growth into a genuine party after the First World War. The precursors of that development, as well as the prewar history of the party, are richly discussed in the essays in this volume.

PETER STANSKY
Stanford University

JAMES E. CRONIN. *Labour and Society in Britain, 1918–1979*. New York: Schocken. 1984. Pp. x, 248. $22.50.

This book aims to contribute to two separate but related enterprises. James E. Cronin writes primarily as a historian, examining a period of British labor history that he claims, with some justification, is underresearched. It comprises also an examination of the sociological investigations of class that have been undertaken since 1945. At the same time, he engages in the continuing discussion of the loosened relationship between the working class and the Labour party, which culminated in the electoral disasters of 1979 and 1983.

Apart from an introductory chapter, Cronin's approach is essentially chronological. His work, however, is not only descriptive but also liberally sprinkled with interpretive comments, and he keeps his eye on his main theme, the connection between social class and politics. Most of his sources are secondary, often unfamiliar works—especially those for the 1920s and 1930s—but some are documents from the Public Record Office and Mass Observation. As is normal these days in labor history, he goes well beyond institutional matters and includes working-class culture as well as industrial (including workplace), economic, and political affairs. As is to be expected, he places the sociological surveys within their historical context; he casts doubt on the validity of some aspects of the recent debate. Thus, after noting that trade union growth in the twentieth century has been "fitful and uneven," he comments, "No timeless model linking class membership to membership in the unions could make sense of that pattern" (p. 8). His instinct is to write as a historian.

Cronin goes on to reject the views of other contributors to the debate by arguing that the working class has not been "bourgeoisified." The Labour party has been losing not because of the adverse effects of social change but because it has failed to attract the growing numbers of white-collar workers and women in the work force. Indeed, those changes, by providing more potential Labour supporters, should have worked in Labour's favor. He does not specify the nature of the program that Labour ought to have introduced, and skeptics might wonder whether electors vote for, or against, party manifestoes or whether they vote for other reasons such as party "image."

Moreover, something is wrong with his analysis in the light of his figures on page 205, which give details of Labour support, according to certain categories, at the general elections from 1964 to 1979. The table shows the expected decline among the party's traditional working-class supporters, but it also shows increasing support from "managerial,

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administrative and professional employees." Some white-collar workers have been attracted to Labour, and they cannot all be polytechnic lecturers or social workers, the mythological militants of the media.

Historians of modern Britain will find this a useful book despite errors from poor proofreading and the absence of a bibliography. It is not quite a textbook and certainly not one for beginners, for it has too many allusions without explanation (for example, to the datum-line strike of 1919). But the book brings together a mass of material and provides an interesting analysis and a connected narrative, only partly superseded by the recent publication of Hugh Clegg's much fuller history of the trade unions, which goes up to 1933.

HAROLD POLLINS
Ruskin College
Oxford, England

B. J. C. MCKERCHER. *The Second Baldwin Government and the United States, 1924–1929: Attitudes and Diplomacy.* (International Studies.) New York: Cambridge University Press. 1984. Pp. xii, 271. $44.50.

In his polished monograph B. J. C. McKercher analyzes how Stanley Baldwin's second government dealt with the growing tension in Anglo-American relations. Despite its title, the book focuses narrowly on the issues of naval rivalry and neutral versus belligerent rights in wartime. McKercher argues persuasively that these issues, which had Foreign Office and Admiralty officials consider the possibility of war with the U.S., were the most explosive ones. Based on extensive research in British private and governmental archives, this book presents a detailed picture of policy disputes and decision making within the Baldwin government. The author's deft portraits of the leading Foreign Office experts on America demonstrate that these men commanded a comprehensive understanding of the U.S. scene. Indeed, many of them had married American women. McKercher's sympathies lie clearly with these British diplomats, and he describes, with loving detail, how they outmaneuvered the divided and amateurish American government on the blockade claims controversy. This was a dispute arising from American shipping losses from the British blockade in the Great War.

London officials met frustration, however, when in 1927 they tackled the issue of limiting the number of naval crusiers. McKercher points out that underlying the cruiser issue was the larger one of neutral versus belligerent rights. In planning future contingencies, both British and American leaders were caught by the past. They envisioned a replay of 1914–17, when Britain was a belligerent and the U.S. was neutral. A large American cruiser fleet, intent on enforcing neutral rights, could threaten Britain's ability to blockade the enemy. Much of McKercher's book deals with the Baldwin government's debate over this problem. By late 1928 the Foreign Office's preference for compromise with the Americans had won out over the harder line, espoused by the Admiralty (and endorsed by Winston Churchill, chancellor of the Exchequer), of building what was needed for imperial defense and ignoring American objections. Thus, the Baldwin government prepared the way for the Labour government's naval rapprochement with the Americans in 1929–30.

Within its restricted purview, this is a model monograph. Yet McKercher's focus on "purely political diplomatic relations" (p. 3) slights the impact on British policy makers of their nation's sharp economic rivalry with and inescapable dependence on the American giant. In the 1920s, Britain could not match America's economic growth or competition. That fact cramped its ability to respond to the U.S. naval challenge. British leaders assumed that in a future war they would again need American credits and supplies. That assumption underscored Britain's ultimate dependence on the U.S. This was the context in which the second Baldwin government concluded that it had to compromise with America.

FRANK COSTIGLIOLA
University of Rhode Island

WILLIAM MCGUCKEN. *Scientists, Society, and State: The Social Relations of Science Movement in Great Britain, 1931–1947.* Columbus: Ohio State University Press. 1984. Pp. 381. $22.50.

What are the social relations of science? Do they mean how science affects society, how society affects science, how science and scientists can aid governments, or how governments can use and control science? In William McGucken's survey of England in the period 1931–47, it apparently means all four.

In the nineteenth century, scientific professionalization meant the divorce of science from normal political and social concerns. Therefore, the British Association for the Advancement of Science (founded in 1831) saw its social role as spreading science to the literate public and was disturbed when the newly admitted statistical section applied its subject to social betterment. For this same reason the Royal Society of London slowly eliminated noblemen who had a voice in the House of Lords and politicians from its elected fellowship. Later in the century scientists limited their social roles to improving education, although the Royal Society continued its long-standing role as adviser and even administrator for successive governments.

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The twentieth century saw slow change: wars, economic depressions, the rise of Fascism, and the publicity of Soviet Russia produced new demands and new organizations. The British Science Guild was founded in 1903 to promote science generally; the Association of Scientific Workers was originally a trade union, which then adopted the same aims as the guild; and the Parliamentary Science Committee, established in 1930, was sponsored jointly with the previous two groups by the British Association. A number of more or less left-wing scientists—ranging from hard-line Communists to Christian Socialists and from Nobel Prize winners to journalists, including the editor of *Nature*—attempted to promote the social relations of science in all ways. McGucken here is not clear about the scientific scene and, as displayed in his bibliography (curiously placed in the epilogue), leans heavily for interpretation on avowedly left-wing writers such as Gary Wersky (*The Visible College* [1978]).

War in 1939 brought the chance for scientists to take part in government, although not without official and unofficial difficulties, and much more could have been written about this in relation to individuals and official bodies. During this time the Left's campaign for state control of science produced a mildly right-wing backlash in the short-lived Society for Freedom in Science, which died because left-wing influence was not really important. The immediate postwar period naturally involved readjustment and reorganization, although, as McGucken does not say, not a return to nineteenth-century laissez faire. He concludes on a pessimistic note, but British scientists, for the next thirty years at least, were very visible on the political scene, and scientists have not ceased to try to explain science and its potential to the public, in ways ranging from television to the Royal Society's recent publication *The Public Understanding of Science*, not to mention the vast number of councils and other bodies that employ scientists and use them to formulate policies.

What emerges from McGucken's work is that there was not one "social relations of science" movement but many movements, not one cohesive policy by scientists but many policies. For, although scientists are a more cohesive and articulate group in public matters than, say, historians, they are also social individuals and therefore speak with many voices. This interesting, though far from pellucid, account of historical events uses many original sources and is a useful introductory work.

MARIE BOAS HALL
University of London

MARGARET DICKINSON and SARAH STREET. *Cinema and State: The Film Industry and the Government, 1927–84*. London: British Film Institute; distributed by the University of Illinois Press, Champaign. 1985. Pp. 280. Cloth $33.95, paper $16.95.

This work details the various efforts of government to assist the chronically anemic British film industry against American domination. Beginning early in the century, Hollywood adopted restrictive economic practices, such as blind and block booking, that helped assure control of the lucrative British market. By the middle of the 1920s Britain produced only 5 percent of the films exhibited on its screens. In part a response to this crisis, the Cinematograph Films Act of 1927 mandated that a certain percentage of all exhibited films be domestically produced. With imaginative cynicism, however, Hollywood circumvented the quotas by acquiring for its exhibitors cheap, rapidly produced British films—the infamous "quota quickies"—which satisfied the law but frustrated its intentions. Amended a decade later, this ineffective legislation became the first of a series of interventions that, by imposing quotas or establishing subsidies, may have rescued British film from extinction but could not restore it to health.

Margaret Dickinson and Sarah Street ably chronicle each special commission, urgent proposal, and government regulation that sought to make the British industry domestically significant and internationally competitive. Although they acknowledge the cultural factors that animated reform, they restrict their analysis largely to the economics of the film trade and, like many of the reformers whose efforts they trace, argue that unfair economic practices, rather than unpopular films, crippled British cinema for most of this century. They constantly imply that many difficulties might have been mitigated if only the government had exercised more thoroughgoing, independent control over production and distribution rather than capitulate to the narrow, self-serving demands of British commercial film interests.

Within the circumscribed boundaries of their self-defined subject, Dickinson and Street have constructed a valuable narrative of the complicated economic and political relationship between cinema and state in Britain. Yet the persuasiveness of their analysis often hinges on the belief that, in matters of culture, supply creates demand. Clearly, the American film industry continually pursued economic strategies in Britain that unfairly restricted competition; in turn, the British government, in close cooperation with its commercial film makers, sought means of limiting the economic and cultural impact of a powerful foreign industry. But audiences in Britain were not passive receptacles of whatever images happened to appear on the screen. It is altogether possible that American films gained ascendancy in part because the public in Britain pre-

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ferred Hollywood productions over most, though not all, domestic commercial productions and certainly over the more uplifting or experimental fare that cultivated elites thought best for them. By excluding cultural factors from their analysis and by accepting uncritically the standard denunciations of commercial film, Dickinson and Street have adopted an approach that omits the role of economic demand in the film industry and quietly patronizes the cultural tastes of the vast majority of British citizens.

D. L. LE MAHIEU
Lake Forest College

MYRIAM YARDENI. *Le refuge protestant.* (L'Historien, number 50.) Paris: Presses Universitaires de France. 1985. Pp. 244. 145 fr.

Le refuge protestant is part of the series L'Historien, which is directed by Roland Mousnier, addressed to general readers as well as historians, and devoted to what we may call position papers by experts on various historical subjects. In this volume Myriam Yardeni summarizes the major aspects of the diaspora of French Calvinists that accompanied the revocation of the Edict of Nantes. She has produced a handbook, concise and easy to read, that is light but not lightweight.

The book opens with a survey of the diaspora and of seventeenth-century French Protestantism, proceeds to describe the reception and establishment of French Calvinist refugees in the Protestant states that took them in, considers the process and problems of assimilation encountered by Huguenot individuals and communities, and concludes with an attempt to connect the Huguenot diaspora with the modernization of Europe, not to say the whole Western world. The author draws almost exclusively on printed sources and secondary works, chiefly in French and English, sometimes in German. Although she makes occasional references to manuscript materials, these are incidental to the main purpose of the book—namely, to synthesize past and present scholarship on the Huguenot refugees. In this sense the book fills a need, for there has been no recent survey of the French Calvinist diaspora as a whole. The study's usefulness for students of history would be greater, however, if the footnotes were more numerous. As it is, the reader will find it practically impossible to follow up either the author's facts or her speculations, except by going to the bibliography and proceeding by guesswork.

The book's other claim to originality lies in the author's discussion of the—often involuntary—acculturation and assimilation of the Huguenots to their host societies and of the ways in which these processes were affected by differences among the states that offered refuge. Twentieth-century sensitivity to the plight of refugees, together with sociological and psychological insights, here adds a fresh perspective to an old story. The weakest part of the book is its final section, on connections between the Huguenot diaspora and modernization. Forty-five pages are inadequate to define, much less explore, the concept of modernity, the question of the Protestant ethic and the spirit of capitalism, the "modern" phenomena of individualism and social and geographic mobility, and the coming of the Enlightenment. True, the author is careful not to claim too much for the Huguenot refugees under any of these rubrics, but the whole discussion seems forced and remains unconvincing.

The book is marred here and there by errors of oversight. For example, it is disconcerting to read that the Huguenots who migrated to South Carolina settled in Charlestown (p. 99), when obviously Charleston is meant. A more important defect, however, is substantive: although the author indicates some of the areas where fruitful work might be done, she does not discuss the kinds of manuscript material available for exploration.

RUTH KLEINMAN
Brooklyn College
City University of New York

HERBERT DE LEY. *The Movement of Thought: An Essay on Intellect in Seventeenth-Century France.* Urbana: University of Illinois Press. 1985. Pp. x, 165. $19.95.

Intellectual and cultural historians who have viewed the seventeenth century as a period of transition in Western Europe have variously described the change as a shift from the baroque to the classical, a scientific revolution, a skeptical or intellectual crisis. Recently, a new critical vocabulary has been developed to analyze a process of change involving the entire culture. Michel Foucault's "epistemes," Thomas Kuhn's "paradigm change," and Timothy Reiss's "analytico-referential" suggest large intellectual movement that can apply to a wide range of cultural artifacts.

Herbert De Ley invokes Kuhn and Foucault in an attempt to chart epistemological change in France from 1557 to 1719 through readings of nine pairs of texts, including poetry, drama, fictional prose, philosophy, mathematics, and fencing manuals. His comparative method challenges both the traditional chronology of literary history and the notion of literary influence. He finds, for example, classical elements in Henri de Saint-Didier's fencing manual dating from baroque times (1573) and baroque elements in Girard Thibault's manual (1628), which was chronologically closer to classicism. Following both Foucault and Kuhn, De Ley sees individual or

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stylistic variations as symptomatic of epistemological shifts.

De Ley adopts Foucault's view that in seventeenth-century France an episteme of representation and classification replaced an episteme of resemblance. He uses Kuhn's model of crisis and assimilation to compensate for Foucault's insistence on discontinuity in epistemic change. Thus, he explains the coexistence of baroque and classical elements in various texts as a tension between two different epistemes, and he sees French classicism as an assimilation of the new paradigm of representation. Classicism, he suggests, resolved the epistemological crisis of seventeenth-century France by repressing one term of the conflict—resemblance—and "normalizing" literature, or returning it to a single model of truth.

De Ley's epistemological concerns lead him to produce new readings of old warhorses such as Montaigne, Racine, and La Fontaine. Because, however, his amalgamation of Foucault and Kuhn is built on a largely uncritical acceptance of their respective categories, we learn more about each text he analyzes than we do about his stated subject, the movement of thought in seventeenth-century France. De Ley, like his mentors, makes no claim to provide social or economic motives; his claims are supported only by his theoretical models. The result of modulating Foucault according to Kuhn and applying the result to a disparate corpus of texts is not so much a new insight into intellectual change as an illustration of current epistemological theory in textual analysis. De Ley's conclusion that by the eighteenth century an earlier unity of truth had broken down and literary truth now depended on verification from the outside apparently rests on Foucault's contention that the seventeenth century was characterized by mathesis. How did the reorganization of knowledge and truth into separate scientific and artistic domains in the eighteenth century arise from the epistemic conflict that De Ley's readings uncover? This question has everything to do with the problem of intellectual change in seventeenth-century France and is not addressed in the book under review.

ERICA HARTH
Brandeis University

FRANCES MALINO and BERNARD WASSERSTEIN, editors. *The Jews in Modern France.* (Tauber Institute Series, number 4.) Hanover, N.H.: University Press of New England, for Brandeis University Press, Waltham, Mass. 1985. Pp. xii, 354. $30.00.

Despite the excellence of some individual contributions, this collection of essays reflects very little of the history, vitality, scholarship, and introspection of the Jews of modern France. Although the major portion of the volume is concerned with French anti-Semitism, Frances Malino and Bernard Wasserstein's attempt to classify the book as a work about Jews led them to provide a misleading title and set of subdivisions for the table of contents. The dust jacket promises that the book will "analyze the history of the Jewish minority in France," but the first essay dismisses not only the historical relevance of the Jewish question in French society but the field of Jewish history as well. Since "Jews are simply humanity writ more clear," Eugen Weber asserts, "the Jewish question can be left to Jews and antisemites, and we may shift from the Jewish condition to the human condition" (p. 27).

Nevertheless, six of the fifteen chapters do deal with the Jews themselves. The two excellent bibliographical essays (by Georges Weill for the period 1789–1870 and by Paula Hyman for the period since 1870) are basic tools for the newcomer to the study of modern French Jewry. The other four—by Richard Cohen, Nancy Green, Michel Abitbol, and Shmuel Trigano—provide important and thoughtful analyses of specific themes.

Cohen offers a concise summary of the dilemmas of French Jewish leadership in responding to Vichy and German demands. In a balanced analysis avoiding earlier extremist views that labeled these leaders collaborators, he describes their attempts and inevitable failure to alleviate Jewish suffering.

Green sensitively analyzes the tension between acculturation and the preservation of independent identity among the East European Jewish immigrants, who created both their own religious institutions and their own separate labor movements.

In his study of French Jewish attitudes toward North African Jewry, Abitbol distinguishes between the French rabbis' method of authoritarian rule, which sought to impose the French model, and the approach of the Alliance Israélite Universelle, which showed greater respect for local leaders and sought to work through them.

The work of Trigano, one of French Jewry's more interesting young thinkers, here becomes available to the English reader. As Stanley Hoffmann says in his response, this essay is best "read . . . as a consciousness-raising exhortation" (p. 287). It calls for a rejection of the civil compact between France and its Jewish citizens created by the emancipation and for the development of a new collective Jewish existence, whose nature is not fully defined, although the author specifically rejects "ghettoization." Steeped in religious tradition himself, Trigano reluctantly recognizes that the revived interest in religious philosophy among French Jews affects only small numbers and that most such feeling is "based more on nostalgia . . . than on Jewish tradition" (p. 258).

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Since the revolution, France and French Jews have experienced an ambiguous and ambivalent relationship. If anti-Semitism persisted in at least some segments of French society, an embracing welcome (whose assimilating potential threatened Jewish continuity in yet another way) was simultaneously extended.

Contributors to this volume line up on two sides of the debate about the extent of French anti-Semitism. On one side are those who study its history and find in their research cause for ongoing concern. Thoughtful and scrupulously researched studies by Pierre Birnbaum and Zeev Sternhell document aspects of the history and continuity of anti-Semitism in the far Left and far Right, making available to English readers some relevant themes from their French-language publications. David Landes emphasizes that both anti-Semitism and assimilation have been constant threats to Jewish existence in modern France.

In contrast, Patrice Higonnet and Hoffmann downplay the significance of French anti-Semitism and do not share the concern of those French Jews who, in seeking a specific French Jewish identity, actively reject assimilation as a solution to Jewish problems. Higonnet, for example, chastises scholars who "dwell on the darker side of the French character" (p. 201). As examples of successful French Jewish integration, he offers assimilated individuals that the Jewish community cannot accept as models, including Max Jacob, the famous convert and Christian mystic. Michael Marrus, in his article on the contemporary period, cites public opinion polls as evidence of a decline in French anti-Semitism. Despite his acknowledgement of the role of French fanatics in some of the recent anti-Semitic incidents, and although he admits a lack of hard evidence in these cases, his "feeling" is that most of the violence is attributable to Middle Eastern terrorism.

Honest men may differ on the extent of French anti-Semitism. Stephen Schuker's outrageous article, however, adopts the anti-Semitic tactic of blaming the victims for provoking hatred. He faults specifically the left-wing politics of East European Jewish immigrants, whom he vilifies for their supposed support for Soviet over French interests. Schuker rejects the research of David Weinberg and Richard Cohen, who have shown that Jewish Communists broke ranks with the French Communist party in September 1939 by urging France to enter the war. He refuses to see in Jewish resistance activity anything more than a subservience to Moscow.

Schuker's editors might have saved him from some egregious errors of fact and misrepresentation of Jewish history and tradition. It was not Napoleon but Louis-Philippe who "recognized Judaism as a religion deserving state support" (p. 147). Statistics on affiliation with consistorial synagogues are no measure of Jewish identity or assimilation (p. 153). French-born Annie Kriegel was never a "foreign adolescent" (p. 176). Jews critical of their own organizations did not engage in anti-Semitism (thereby relieving anti-Semites of blame), as Schuker implies (p. 149). There is no "chiliastic element in traditional Judaism" (p. 170). This uniquely Christian concept cannot be applied to Jewish thought.

Some of the other essays also contain errors that careful editing could have prevented. Overall, the collection does not well serve those readers not expert in the field, since much current scholarship on key themes is ignored and the resulting picture is skewed.

PHYLLIS COHEN ALBERT
Harvard University

P. M. JONES. *Politics and Rural Society: The Southern Massif Central, c. 1750–1880*. New York: Cambridge University Press. 1985. Pp. xiii, 375. $49.50.

In this study of peasants in the highlands of southern France, P. M. Jones finds little evidence of a political watershed during the Second Republic and much confirmation of Eugen Weber's thesis that rural culture remained impervious to national politics until the early years of the Third Republic. Kinship, territorialism, religion, and patronage are the concepts that Jones deploys to explain the sources of political allegiance within rural society. He brushes aside other explanations of peasant politicization that emphasize market agriculture, urban influence, class conflict, and state repression. Peasants continued to live in an archaic world of patriarchal authority and religious belief long after they had begun to participate in the electoral institutions of the modern, liberal state. At best they experienced cycles of politicization and indifference in response to short-term crises in the economy and the state, such as the Revolution of 1848 and the agrarian depression that accompanied it. Even in the 1880s, when structural changes in communications and schooling began to break down the insularity of rural culture, peasants voted Republican from expediency rather than conviction and became opportunistic instead of hostile to the external world of towns and the state. Therein lay their range of choice and their modernity.

Jones develops his general argument within the context of a particular region—the southern Massif Central—whose social geography, rural economy, family structure, and cultural values were deeply influenced by a harsh natural environment. Poor communications and infertile soil hindered the development of towns in this region where peasants practiced a traditional mode of agriculture based on

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extensive common lands, dispersed hamlets, and patriarchal households. Jones emphasizes the distinctive cultural features of this peasant society, whose authoritarian families fostered discipline, piety, and deference.

Having analyzed the enduring feature of rural communities in the southern highlands, he turns to the political history of this region between the French Revolution and the 1880s. His chapters on the revolutionary decade of the 1790s couple a rather conventional description of popular "atavisms"—sectarian violence, fiscal aggression, and agrarian uprisings—with a more original analysis of territorial rivalries and factional conflicts that gave birth to new ideological traditions. In a subsequent chapter on the "democratic challenge" in the nineteenth century, Jones breaks new ground by analyzing how voting procedures expressed the solidarity of rural communities and kinship groups. He also highlights the emergence of the Catholic church as a powerful electoral organization in a region where sectarian conflicts between Catholics and Protestants had a profound impact on rural politics. The transposition of religious loyalties into political allegiances reached its peak in the 1880s and then subsided, as peasants acquiesced in what Jones describes as the "milch cow" state. With this cynical label Jones intimates that, for Republican politicians and peasant voters alike, modern politics panders to interests, not ideology.

This book will impress the general reader with the clarity of its argument and the specialist with the range of archival and printed sources used, the solidity of its regional analysis, and the breadth of its comparative remarks about other monographs. Jones perhaps rests his argument on the most backward and isolated areas within the region that he has chosen for study, and he tends to replace a one-sided account of peasant radicalization with an equally debatable interpretation of cultural archaism and political conservatism. At a general level his book is challenging but not conclusive. In this respect it mirrors the diversity of French rural society, wherein each family and community possesses its own history but peasants in general have no collective memory.

TED W. MARGADANT
University of California, Davis

JILL HARSIN. *Policing Prostitution in Nineteenth-Century Paris*. Princeton: Princeton University Press. 1985. Pp. xxv, 417. $43.50.

Jill Harsin's important monograph is a welcome addition to the growing body of literature on prostitution in Europe and on the interaction between public authority and the private lives of working-class individuals in nineteenth-century France. Single women, prostitutes perhaps more than others, have felt the intrusion of the state into their lives since the early decades of that century. Harsin focuses on the attempts of social hygienists, moral reformers, government officials, and the omnipresent police to control and regulate the behavior of Parisian prostitutes and working-class women suspected of prostitution.

The strength of this book lies in illuminating the lives of the prostitutes. They appear as initially respectable working women who, through economic necessity, turned to prostitution. Gradually, many fell completely into the milieu of the "dangerous classes" and became separated from respectable working-class life. Prostitutes were boisterous and committed suicide, petty crimes, and acts of violence. They also were victims of violence. Arrest was frequent and routine; prostitutes, and working-class women suspected of prostitution, were outside the law and denied civil liberties once they were arrested. Harsin's meticulous use of police records in the Archives Nationales accounts for her vivid picture of the criminality of prostitutes. She also sensitively depicts prostitutes as vulnerable, separated from friends and family, and victims of disease, public authority, police, madams, and customers.

Alexandre J. Baptiste Parent-Duchatelet, on whom Harsin relies too heavily and to whom she devotes an unnecessary chapter, suggests that prostitution may have been a passing stage in the lives of some poor women. Although Harsin examines the professionalization of prostitution, one weakness is her failure to examine closely the issue of prostitutes' separation from, and possible reintegration into, working-class life. If society regarded them as criminal outcasts, as Harsin proposes, then it seems that their return to working-class life would be problematic, especially for the registered prostitute. Their criminal marginality, however, might make the resumption of respectability possible.

This book tantalizingly suggests the socioeconomic and demographic background of the prostitutes, but the data are mostly from Parent-Duchatelet. Crucial questions remain only partially answered. What separated prostitutes from other poor and friendless women in the city? Was it economic failure alone? Did an illegitimate child or one arrest for disorderliness or suspicion of prostitution force a woman to prostitution? Did prostitutes have some common family background that distinguished them from other working-class women? Did police persecution lead prostitutes to form a support network or subculture? These questions are difficult for any historian to answer, and Harsin does a fine job with the published sources and police records on which she relies. But possibly

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the unexplored records of Saint Lazare, Bon Pasteur, and Petite Force or even those of charitable rescue societies would provide useful information on the women's strategies.

Third Republic attitudes toward prostitutes and regulations governing their lives changed from those of earlier regimes. Public authorities came to view the women more as victims and less as dangerous criminals. Greater analysis of the reasons for the change and its relationship to other events would be helpful. The police regulatory system of prostitution ended just after the repeal of the Contagious Diseases Acts in Britain and simultaneously with the development of a social welfare system in France that included an elaborate inspectorate intruding on and regulating the lives of the poor.

This lucid and deftly written monograph has enriched our understanding of nineteenth-century attitudes toward women, their position in Paris, and the influence of the state on women's lives.

RACHEL G. FUCHS
Arizona State University

ANN-LOUISE SHAPIRO. *Housing the Poor of Paris, 1850–1902*. Madison: University of Wisconsin Press. 1985. Pp. xx, 224. $32.50.

The growth of major cities has almost invariably been marked by disequilibrium between the rate of immigration and the expansion of the housing stock. Historically, the urban immigrant, usually a young adult destined to remain in the city throughout his or her life, has been the product of a rural expulsion process determined by difficulties in securing a livelihood from the land. Or, perhaps less frequently, the immigrant was lured by the real or imagined prospect of prospering in a burgeoning city economy. Once arrived, the immigrant nestles into ghettoes of his or her own people in the cheapest and most squalid conditions as near as possible to the source of employment. The product of poverty, indifferent to proper sanitation and standards of cleanliness prevalent in more affluent urban society, the immigrant is rapidly identified with the decay of the inner city. The wealthy flee to the more spacious suburbs while the continued rural influx into the crumbling inner-city slums accentuates the polarization of the city into affluent and poverty-striken zones. Ironically, the greatest landlord profits are to be found in the latter, because people with few resources can be crammed into an ever-diminishing space. Such landlords are interested neither in property improvement nor in the creation of newly constructed living quarters, for their income depends on demand sustained by outstripping supply.

Such generalizations are applicable to twentieth-century Calcutta, London, Boston, and Detroit but were manifest in nineteenth-century Paris because railroad development brought the provincial poor into the capital. Yet the situation was uniquely highlighted by Paris's record as the cradle of revolution in which the inner-city poor with economic grievances were arbiters, if not architects, of political change and capable of effective intrusion on the political scene. Advances in knowledge of microbiology and hygiene that developed by leaps and bounds in postcholera Paris located infection in crowded, unsanitary dwellings, the nursery of bacteria. Furthermore, a critical debate was afoot about the desirable limits of state intervention in the lives of the citizenry. To what extent should the state intrude to impose standards on landlords and tenants or to attempt to increase the housing stock? France was traditionally a country that looked to private philanthropy rather than to state-based assistance, and French politicians of a liberal or Roman Catholic persuasion argued respectively for laissez faire or individual Christian initiative to solve social problems. Even those socialists committed ultimately to state control of all aspects of economic life were ambivalent about too wholehearted a commitment to the debate on improved housing, which they saw as a detraction from the main business of securing a better wage for the worker.

Ann-Louise Shapiro's study is neat, nicely constructed, and tightly argued. It is essential reading for anyone interested in the problem of the urban immigrant in a laissez-faire economy as well as for the student of nineteenth-century Paris. The author has wisely refused to be distracted by wider issues—for example, linking housing conditions to revolution or allowing herself to be engulfed by the great debate between socialists and Catholics on the responsibility of the state and the individual, which marked the twenty years before 1914. Instead, she focuses on the moral, political, and scientific pressures for improvement and why they achieved so little. The study adds a new dimension to the works of Louis Chevalier, Anthony Sutcliffe, and the wide-ranging Theodore Zeldin, and Shapiro deserves praise for the scholarly, compassionate, but never sentimental treatment of a complex issue.

OLWEN HUFTON
University of Reading

DONALD REID. *The Miners of Decazeville: A Genealogy of Deindustrialization*. Cambridge: Harvard University Press. 1985. Pp. vi, 333. $25.00.

Labor history is often compartmentalized into studies of unionism, work experience, and industrial development, with little mixing of the genres.

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Donald Reid's recent book is a welcome exception. Although his title mentions labor alone, Reid also examines changes in management style, problems of production and technology, and growing state intervention in French coal mining, from before 1800 to the present.

The mining industry is well suited to this blend of topics because of the miners' initial militancy and later reformism, the large companies' early development of modern management techniques, and the state's ample role both as regulator and as spur to productivity. The choice of Decazeville aptly targets a town literally created by new industry, a "company town" in which managers long controlled local commerce and even political life. The lengthy period studied allows Reid to survey the rise and decline of local industry, a survey emphasizing structural changes but punctuated with tales of strikes and other dramatic events.

Reid's main argument, as his subtitle suggests, is that "deindustrialization" had human consequences comparable to the better-known effects of industrial progress. Economic difficulties, which finally prompted the mines' closure in the 1960s, had led managers to tighten control over the labor process and unions to evolve a "national political strategy" (p. 128) for winning legislative benefits rather than direct gains from employers. The economics of deindustrialization are, however, less fully described here. Nor is it clear how unique these effects were to this "declining region" (p. 148), since miners elsewhere in France experienced similar pressures and developed comparable strategies in national unions such as the Confédération Générale du Travail. Reid implies that miners in wealthier regions such as the Nord could act on their own and place less value on nationwide organization. The idea that labor strategies vary with levels of regional prosperity or with phases in a "genealogy of deindustrialization" is intriguing and might be more explicitly advanced.

Perhaps the point is that in this book politics takes second place to the sociological consequences of a process that created a distinct mining proletariat, an intermediate cadre of engineers and supervisors, and an elite of managers or directors who turned from paternalism to industrial rationalization. Just as important to this story as the miners' debates over rival union strategies is the emergence of separate unionization among engineers and supervisors, neither prolabor nor fully promanagement in viewpoint: they opposed the strikes of 1947, which compromised productivity, but then took labor's side in those of 1961–62 against the shutdown of the mines.

The book's sociological dimension is further strengthened by an appendix that draws a social profile of nineteenth-century miners, demonstrating their occupational identity as distinct from local metalworkers and unskilled laborers. Indeed, the emphasis here on nineteenth-century developments, before deindustrialization had fullest impact, shows the book's wider focus on miners' attitudes toward their industry, their town, and their country. Reid's study of deindustrialization raises timely questions about state responsibility toward disadvantaged regions but does not neglect longer-term issues that are of equal concern to historians.

KATHRYN E. AMDUR
Emory University

GERVASE CLARENCE-SMITH. *The Third Portuguese Empire, 1825–1975: A Study in Economic Imperialism.* Dover, N.H.: Manchester University Press. 1985. Pp. ix, 246. $29.50.

Portugal experienced three cycles of imperialism. The first, expansion in Asia in the sixteenth century, shortly left her mere remnants of colonies. The second cycle began with the colonization of Brazil, which, until its independence in 1825, offered more enduring opportunities for occupation. Although Portugal established a presence at scattered entrepôts centuries earlier, its third empire commenced with the scramble for Africa in the late nineteenth century. With its occupation of the interior of Angola and Mozambique, Portugal attempted to create a new Brazil.

In the preface, Gervase Clarence-Smith poses a problem: why were the Portuguese in southern Africa? If imperialism is viewed as an expression of domestic politics, another question surfaces: how did imperialism redistribute power among the various classes in Portugal? The hypothesis is that the driving force behind imperial expansion was the search for markets, or neomercantilism.

The second and third empires are particularly interesting examples of subimperialism, expansion under the aegis of a greater power. Sadly, the monograph deals lightly with the relationship—sometimes cooperative, sometimes adversarial—between dominant Britain and subdominant Portugal.

The first third of the monograph describes commerce in the Portuguese-speaking world before 1850. The circulation of goods, capital, entrepreneurs, and labor within this far-flung world is sketched through some biographies. The middle third deals with the occupation of the African interior, from 1850 to 1926.

The last third of the monograph offers interesting insights into the Salazar period (1926–61) and the breakdown of colonialism (1961–75). The intransigence of the Salazar regime toward colonial rebellions is attributed to the interest of the regime in its own survival rather than to economic gain, which is contrary to the author's working hypothe-

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sis. In the last twenty years of colonialism, conflicts among white industrial, working-class, and peasant interests in various parts of the empire intensified. The third empire became increasingly valuable to some industrialists in traditional industries, but a liability to those wishing to link Portugal's newly industrializing economy to Europe. The interrelationships between metropolitan industrialization and decolonization are sketched all too briefly.

Unfortunately, Clarence-Smith lacks a coherent conceptual scheme. The hypothesis that Portugal was driven by a neomercantilism is not cast in a verifiable manner. Indeed, the fragmentary evidence is not supportive. As indicated by trade data, the colonies rarely absorbed more than one-quarter of Portugal's exports. Their worth as a significant safety valve for surplus labor was insignificant, for the majority of emigrants went to Brazil, the United States, and France. The returns on capital investment appear to be modest, at least until the 1950s. Fiscally, these colonies were subsidized by taxes from Macao, except during the 1940s and 1950s.

The author offers no convincing scenarios of what would have happened to Portugal in the absence of imperialism. Small, nonimperial countries, such as Sweden and Denmark, developed rapidly during the scramble for Africa. Was imperialism a substitute for reform and an obstacle to economic development? An examination of income from Africa to the various groups in the metropole would have been welcome.

Clarence-Smith's contribution to the historiography of imperialism is modest. One wishes that more emphasis had been placed on the subjects of the last two chapters, which are superb.

MARTIN T. KATZMAN
University of Texas, Dallas

MICHAEL MEYER. *Strindberg*. New York: Random House. 1985. Pp. xvi, 651. $24.95.

Michael Meyer is known for his biography of Henrik Ibsen, and on the back cover of his new book about August Strindberg the Ibsen volume is described as one of the truly great biographies by ten reviewers, none of whom, with the possible exception of Eva Le Gallienne, is an established Ibsen scholar. Meyer surely is not: most Scandinavian specialists will find that, rather than being praised for his original scholarship, Meyer should in both these books be viewed as an editor, writer, translator, and literary critic. Strindberg, we are told, is one of history's seven internationally great dramatists and, among them, possibly the one who led the strangest life.

Nevertheless, a substantial biography in English has not been available until now. That this book, written by an Englishman for English readers, now exists is in itself remarkable. To a non-Swede, Meyer's account of Strindberg's early years will appear quite full, although naturally in a book aimed at an international audience the author emphasizes the playwright's European life, exile abroad (including his years of near-insanity in Paris, where he tried to turn sulphur into gold) and final years in Sweden, where he died in 1912. We hear of his three (almost four) stormy marriages, his cruel treatment of friends and enemies, his innovative painting and photography, and, of course, his literary production.

Meyer's account is lively throughout; indeed, for anyone unfamiliar with Strindberg's life this book is difficult to put down. There are also few omissions and few factual mistakes. Meyer writes about Strindberg's anti-Semitism, but nowhere in this 650–page book does he mention Ernst Josephson, Sweden's greatest painter, of Jewish descent, whom Strindberg admired and with whom he identified. Meyer also includes Norway's best-known novelist, Knut Hamsun, in the Black Porker circle in Berlin, which is perhaps a forgiveable mistake because Hamsun intended to join the Black Porkers but never did.

For a literary biographer, Meyer often seems lacking in critical insight but nonetheless arrogant in his judgments. He tells us outright that Strindberg's poetry is overrated and that his novels, with the exception of *The People of Hemsö*, are fatally flawed; this is perhaps an attempt to appear independent vis-à-vis the secondary works. More serious is his lack of admiration for the man, which colors the entire treatment. Even objective observers of Strindberg would be hard put to accept Harriet Bosse's characterization of her husband as "the finest man I have ever known" (p. 571), and, as Meyer points out, the testimony of enemies is as essential a part of any biography as the evidence of friends. But, although a great man may not be likeable, a certain degree of sympathy for him is necessary in any biographer who hopes to treat his subject fairly. An example is the discussion of the character Zachris (in *Black Banners*) as a hateful portrait of Strindberg's old friend Gustaf af Geijerstam: Meyer joins the public in its "indignation [over] this extraordinary and sustained libel" (p. 488), whereas Olof Lagerkrantz, in his recent book on Strindberg (1979), sees Zachris also as a devastating self-portrait. Although in her own biography (1949) Elizabeth Sprigge comes out tirelessly understanding of Strindberg, she writes that "he invites us to dislike him"; Meyer has not been able to resist this invitation. Lagerkrantz is not mentioned in Meyer's five-page bibliography of "works which I have

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found useful," although it is difficult to see how Lagerkrantz's book, in which any invited dislike of the subject has been seasoned throughout by empathy, could not have been a helpful guide to the treatment of this giant of modern drama. Even so, the sheer bulk and captivating style of Meyer's biography makes it more substantial than any earlier attempt to present Strindberg's remarkable life in English.

HARALD S. NAESS
University of Wisconsin

MARILYN J. HARRAN, editor. *Luther and Learning: The Wittenberg University Luther Symposium.* Cranbury, N.J.: Associated University Presses, for Susquehanna University, Selinsgrove, Pa. 1985. Pp. 144. $19.50.

This volume contains six essays delivered at a Luther anniversary symposium at Wittenberg University in Springfield, Ohio. To those interested in the Reformation the essays offer review and stimulation on a group of topics that at least loosely fall under the title of the book.

Perhaps the most provocative and wide-ranging analysis of Luther's thought comes from Daniel Olivier, the French Luther scholar. He persuasively argues that Luther contested not merely the medieval view of the relationship between works and salvation but also, far more significantly, the rational framework that Roman Catholic theology then and now has imposed on its understanding of the Christian message. Olivier focuses on the importance of Luther's doctrine of sin for modern use of his theology, and he contends that Luther provides a better basis for approaching positivist rationality than does that traditional Catholic framework.

Lewis W. Spitz carefully analyzes the degree to which Luther was familiar with humanistic works and methods and the ways in which Luther used the humanists' materials and insights in constructing his own theology. Spitz demonstrates that Renaissance humanism contributed significantly to Luther's reformatory thought.

James M. Kittelson's essay on the "educational reformer" continues where Spitz leaves off; it is particularly helpful in assessing the relationship between Luther's view of the theologian's intellectual activity and the views of his students and successors on that subject. Kittelson acknowledges that Philipp Melanchthon and others shifted from Luther's understanding of Christ as the content of the Scriptures to a view of Scripture as a collection of *loci*. But he astutely cautions against the oversimplified view of the late Reformation and orthodoxy that represents the thought of this period as mere intellectualization or that overlooks Luther's own emphasis on pure doctrine and right teaching. One path to clarifying these relationships may well lie in a closer examination of the function of *loci* in the theological practice of Luther's successors.

Marilyn J. Harran views Luther's life as professor from several angles. She cites his own testimony to the importance of his vocation to the teaching office for his call for the reform of the church, and in a very helpful way she surveys the impact that his professional duties had on the course of the Wittenberg reform movement.

Leif Grane's analysis of Luther's continuing relationship to scholasticism shows us how influential scholastic thought remained for Luther throughout his career, in part because of his bitter reaction against the deception that it wrought on the anxious young monk and in part because of his deep respect for the quality of scholastic argument, which made it particularly dangerous as an obstacle to the correct understanding of the gospel.

The volume concludes with Krister Stendahl's reflections on Luther's understanding of the Word of God and its power. Stendahl reveals the large gulf that lies between Luther's view of the Word and that of a contemporary Lutheran exegete, but not without saluting "Luther's marvelous arrogance in achieving a clarity that released a new spiritual energy and a new perception of the meaning of the Word of God, and for his humility in allowing the Word of God to stand in its fullness" (p. 141).

Like every collection of essays, this volume has more and less helpful contributions, but it does offer fresh insights and overviews of significant questions under discussion among Reformation scholars today.

ROBERT KOLB
Concordia College
Saint Paul, Minnesota

JACOB TOURY. *Jüdische Textilunternehmer in Baden-Württemberg, 1683–1938.* Assisted by EVA CH. TOURY and PETER ZIMMERMANN. (Schriftenreihe Wissenschaftlicher Abhandlungen des Leo Baeck Instituts, number 42.) Tübingen: J. C. B. Mohr (Paul Siebeck). 1984. Pp. xiv, 294.

This book, encouraged by the Leo Baeck Institute and funded by the government of Baden-Württemberg and textile industry sources, delivers exactly what the title promises: a series of studies of Jewish entrepreneurs in the textile industry of the regions of southwestern Germany that today comprise the federal state of Baden-Württemberg. Jacob Toury begins with references, found in the proceedings of the city council of Mannheim (1681–84), to Moses Jacob, a supplier of coats and coat material to city officials, and ends with a chapter

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on the progressive elimination and sequestration of Jewish firms in Baden, Württemberg, and Hohenzollern by the Nazi regime between early 1933 and late 1938. In his foreword Toury regrets being unable to mention by name each of the one thousand Jewish textile firms and each of the one hundred twenty places in Baden-Württemberg where they were located, but he cannot have missed very many of them. The index of place names begins with Aalen and ends with Zuffenhausen, and the index of personal and firm names begins with Adelsheimer and ends with Zwirnerei- und Nähfadenfabrik AG (Dietenheim). The former contains 241 entries and the latter 1,195 entries, if my count is correct.

The book's structure and argument are easy enough to criticize. There is no reason to treat Baden-Württemberg as a single unit before the Second World War and no reason to treat either Baden or Württemberg as units before the Napoleonic wars. The districts under Habsburg rule in the eighteenth century are not compared with other Habsburg possessions, and the newly expanded states of Baden and Württemberg in the early nineteenth century likewise are not compared with other German states. Toury defines the textile industry quite broadly, including at times everything from retail trade in clothing to the production of featherbeds. This appears to be based on some notion of entrepreneurial expertise or perhaps on a community of entrepreneurs in the industry, but the connections are not worked out systematically. Similarly, although Toury notes the difficulties of defining the boundaries between wholesale and retail trade and between large- and small-scale production, he devotes relatively little space to analyzing the relationships among these businesses while expending much space to prove that Jewish firms were in fact large. Although Toury states that Jewish entrepreneurs in southwestern Germany tended to concentrate in the textile industry, he does not explain that tendency in detail, either with respect to the specific position of Jews or in comparison to non-Jewish entrepreneurs. Finally, he does not examine the structures of local Jewish communities, although such a perspective might have shed light on some otherwise mysterious differences, such as those between the Jewish firms in the Karlsruhe and Mannheim textile industries in the late nineteenth and early twentieth centuries.

Nonetheless, the book contains a wealth of specific information, which should prove helpful to students of various persuasions. The author includes as well some useful material on the employment of orphan and convict labor in the early eighteenth century and equally useful portraits of individual firms active during the nineteenth and early twentieth centuries. The chapter on the Nazi period is both solid and moving in its portrayal of the gradual increase in pressure on the Jewish community; as elsewhere, those with most to lose tended to be the last to recognize the danger that confronted them. The material also suggests some general points, although these remain largely implicit. The absence of continuity, for instance, is intriguing. There were very few long-lived Jewish textile firms in Baden-Württemberg. As Toury notes, most of the firms did not survive more than a single generation. This suggests that their pattern of rise and fall paralleled that of most non-Jewish firms, but it also raises the question of continued overrepresentation of Jews in the textile industry again. Had the project been less rigidly structured, these and other questions could have been considered more effectively and would have provided the central argument that the book at present lacks.

FRANK B. TIPTON
University of Sydney

ECKHART HELLMUTH. *Naturrechtsphilosophie und bürokratischer Werthorizont: Studien zur preussischen Geistes- und Sozialgeschichte des 18. Jahrhunderts.* (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 78.) Göttingen: Vandenhoeck und Ruprecht. 1985. Pp. 302. DM 65.

Eckhart Hellmuth provides an interesting, provocative contribution to questions of the intellectual origins and character of Prussian absolutism in the second half of the eighteenth century. His main thesis is that top Prussian civil servants were committed to social and political values that supported the paternalistic, absolutist Prussian state. He maintains that these values were derived not merely from Prussian pietism but from secular, jurisprudential sources as well. These secular sources are the focus of Hellmuth's attention. In particular, he establishes links and "affinities" between the natural law theories of Christian Wolff, Daniel Nettelbladt, and Joachim Georg Daries (theories that Wilhelm Dilthey once paradoxically labeled "Prussian natural law") and the political thought of some thirteen high-ranking civil servants (including Christian Paalzow, Christoph Gossler, Ernst Klein, Karl Svarez, and other authors of the famous *Allgemeinen Landrecht* reform begun under Frederick the Great). The most important link was a common place of study at the Universities of Halle or Frankfurt an der Oder, the main centers for the teaching of "Prussian natural law." The intellectual affinities are extensive, but Hellmuth's argument is not without problems. The problems arise partly because of his mode of proceeding and partly because of the constraints imposed by the requirements of dissertations.

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The argument proceeds in two stages. In part 1, Hellmuth examines the views of Wolff, Daries, and Nettelbladt on a number of questions that are central to their systems of natural law: the ends of the state, the forms of government, "patrimonialism," freedom, social structures, and the relationship between law and morality. From this somewhat disjointed analysis, these thinkers (especially Wolff) emerge as much more conservative, paternalistic, and authoritarian than they have often been taken to be. In part 2, Hellmuth considers the views of his civil servants on the same broad questions. Despite obvious differences between them, he persuasively establishes a shared ethos, amounting to something like a common "ideology," which he sees as stemming from Wolff, Daries, and Nettelbladt.

The work is as tight and rounded as one might expect from a good dissertation. But, precisely for this reason, it is too circumscribed to do full justice to the evidence discussed and the important questions raised. Part 1 suffers from the author's refusal to consider his sources within the immediate context of the development of modern natural law, despite the fact that Thomas Hobbes and Hugo Grotius lurk behind almost every page and Samuel von Pufendorf's presence is so manifest. Part 2 is less immediately vulnerable to criticism. Hellmuth is too good a historian to assume that affinities amount to influences. He frequently notes that the meanings of the masters were modified in the transposition to the practical world of their former students. But he does not develop the point. His account remains a good example of a history of influence and effects. Had he followed his own intimations and attempted a history of receptions instead—the reception of natural law treatises by the members of the Wolffian school and the reception of Wolffian texts by a number of Prussian civil servants—his work would have gained considerably more sophistication than his token references to Max Weber and Karl Mannheim provide.

MARTYN P. THOMPSON
Tübingen University

MANFRED AGETHEN. *Geheimbund und Utopie: Illuminaten, Freimaurer und deutsche Spätaufklärung*. Foreword by EBERHARD SCHMITT. (Ancien Régime, Aufklärung und Revolution, number 11.) Munich: R. Oldenbourg. 1984. Pp. 337.

The German post-Annales school of sociologically orientated modernists has strongly influenced Manfred Agethen's stimulating study of the German *Illuminati* and their relations with the Freemasons. The *Illuminati* were based in Bavaria and led by Adam Weishaupt until Duke Karl Theodor's attack in 1785 led to the official dissolution of their secret society. The group's lodges extended throughout Germany, and its membership encompassed many of the most notable intellectuals of the age, including Baron Adolphe von Knigge and J. G. Herder (Weimar).

The modernist school emphasizes the importance of sociological concepts for the interpretation of raw data and often refrains from narrative analysis. In this case the conceptual component used to unlock the data on the *Illuminati* connection among German intellectuals is the idea of a sect. The secret societies of the eighteenth century were not yet political parties but were viewed as a dangerous and organized political opposition, indeed, as heretical "sects."

The persecutory elements in the operations of late eighteenth-century enlightened absolutism are more clearly brought into focus here than in many standard works on the subject, which are primarily concerned with administrative and institutional reform. As secret societies, both the Freemasons and the *Illuminati* copied the hierarchical structure of a monastic order. Agethen never describes the rituals of any of these orders, although he does refer to the grades of membership achieved. Many of the German Freemasons of the 1780s followed the "strict observance" of such rituals, but Agethen never clearly describes them either. Secret societies were attacked by the Catholic clergy since they opposed Catholic doctrine while imitating some features of the church's organization. The *Illuminati* took in both Protestant and Catholic clergy, however, and continued to attack the Jesuits. Surprisingly, some of the leading Catholic statesmen of the old *Reich* had been *Illuminati* or Freemasons. Karl Theodor von Dalberg was an *Illuminato* when he was elected archbishop-elector designate of Mainz in 1787 by twenty-four *Illuminati* canons.

Yet both Karl Theodor of Bavaria and Emperor Joseph II tried to stamp out the *Illuminati* as well as the Freemasons by dismissing officials who were members of these groups and confiscating their property during the 1780s. Joseph II's government also tried to swing Dalberg into the Austrian camp in 1787, but he joined the Prussian Prince's League instead.

The extent to which the secret societies represented not only "sects" but also idealized utopias that expressed the goals of such organizations is identified brilliantly with the reform enthusiasm (*Chiliasmus*) of the Enlightenment. Again brilliantly, Agethen shows that all the contradictions of the times manifested themselves in these societies. The *Illuminati* hoped to reduce social barriers but wound up accentuating the differences between the middle class (confined to the middle ranks of the order) and the upper and intellectual classes, which held the top positions. The insistence on good conduct re-

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ports and the incessant spying also replicated the worst features of the absolute state and made utopian goals impossible to attain.

HELEN LIEBEL-WECKOWICZ
University of Alberta

WOLFGANG ALTGELD. *Das politische Italienbild der Deutschen zwischen Aufklärung und europäischer Revolution von 1848.* (Bibliothek des Deutschen Historischen Instituts in Rom, number 59.) Tübingen: Max Niemeyer. 1984. Pp. xii, 401. DM 108.

The incorporations of Venice and Rome into the kingdom of Italy were obviously by-products of Prussia's wars against Austria and France. But German unification could hardly have come about in 1871 if the upheaval of 1859–60 had not resulted in the unification of the rest of Italy. Between 1859 and 1870, Italy was seen by Germans to be ahead in the unification stakes.

It was a different story earlier, as Wolfgang Altgeld shows in this intelligent and thorough book. German publicists recognized that the problems of the two nations were inextricably linked. Both were politically fragmented; both were backward by comparison with Britain and France; both felt the lowering presence of the Austrian empire, first as the promoter of Enlightenment, then as a bulwark against the revolution and Napoleon, and, after 1815, as the standard-bearer of Restoration. But, however enthusiastically Germans admired the artistic achievement of Italy, they were convinced of the political inferiority of contemporary Italians, who were renowned for laziness, superstition, and decadence. There was no question of Germans taking lessons in politics from Italians.

Censorship limited what Germans could write about Italy, but both the quantity and quality of comment were impressive. Many of the great writers of the period contributed something to it, and the attitudes displayed throw some light on the Risorgimento and a great deal on German political opinion. From the standpoint of Italian history, it is instructive how much impact the revolutions of 1820–21 had on German thinking both when they broke out and when they failed. Leopold von Ranke's account of the work of Ercole Consalvi, published in 1832, so close to the events it describes, has evidently been unfairly neglected by subsequent historians. Support for violent revolution seems scarcely to have existed in Germany: Giuseppe Mazzini attracted little notice and still less applause; for some years his namesake, Andrea Luigi, was better known.

The Italian reality was ultimately less important to German writers than the opportunity afforded by discussion of Italian affairs for implicit comment on the German situation. Views expressed about the papal state or the temporal power defined the writer's shade of Catholicism—or his Protestantism. Opinions about the role of Austria in Italy applied, *mutatis mutandis*, to its role in Germany. The position in the 1840s is particularly revealing. German-Austrian feeling grew, necessarily at the expense of sympathy with the Risorgimento. The veteran Ernst Moritz Arndt, on the other hand, under the influence of the Prussian confrontation with France in 1840, became enthusiastic for Italian regeneration in order to strengthen Germany against the French, although he was convinced that only a "despotic revolution" could achieve the desired result, with "cutting and burning iron" (pp. 255–56). In 1848, German liberals cold-shouldered Italian nationalists and reformers, especially after the inspiring Teutonic victories of Radetzky.

This book, then, provides a much broader picture of German opinion than its title suggests. The volume is excellently produced and contains a most valuable bibliography. The author has his vast range of sources under better control than do most writers on public opinion. But his treatment of all German-language opinion, including Swiss and Austrian, as an entity is naturally at times unsatisfactory.

DEREK BEALES
Sidney Sussex College
Cambridge University

GERHARD A. RITTER. *Die deutschen Parteien 1830–1914: Parteien und Gesellschaft im konstitutionellen Regierungssystem.* (Kleine Vandenhoeck Reihe, number 1507.) Göttingen: Vandenhoeck und Ruprecht. 1985. Pp. 117. DM 14.80.

It is hard to see the point of this thin little book. It purports to place the German parties in their political and social context by exploring their relationship to the determinate constitutional situation and to the impact of basic social, economic, and religious forces. It proceeds from a few familiar nostrums of German historiography, such as the regionally fragmented federalism of the imperial polity, the authoritarian dualism of an elected legislature and an independent monarchical executive, the structural persistence of a fixed five-party system since the 1870s (namely, Socialists, right- and left-wing Liberals, political Catholics, and Conservatives), and the basic immobility of the political system. Ritter includes perfunctory discussions of the emergence of parties between the 1840s and 1870s and of the working of the electoral system. The "core of the book" (as Gerhard A. Ritter calls it) is a discussion of the parties' respective social bases, in which Ritter draws fleetingly and selectively on recent research. The survey concludes with some attenuated re-

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marks on the unreformability of the imperial state directed mainly at the work of Manfred Rauh, who ruffled the feathers of some West German historians in the 1970s with his overstated and poorly specified claims regarding the "parliamentarization" of the empire between the 1890s and 1914.

Ritter has nothing particularly new to say. His idea of the five-party system is a faintly modified version of Rainer Lepsius's old idea of the modernization-resistant "social-moral milieus" on which German political loyalties were allegedly fixed from the 1860s to the 1930s. This notion has always served to play down the importance of movement and change within the overall time span, and Ritter's use of it is no exception. Thus, the reified emphasis on closed political boundaries obscures the extraordinary volatility of the electorate in many parts of Germany in this period. It marginalizes political forces that do not fit easily into any of the main divisions but that may have been regionally and conjuncturally significant, such as the variegated agrarian and anti-Semitic protest movements of the 1890s and later, which Ritter largely omits from consideration. Similarly, Ritter refers to the flux of the 1890s and the entry of new forces into the political arena but mainly to minimize their significance: he makes no effort to engage recent arguments regarding the changing bases of political mobilization. Despite a rhetorical gesture toward the grass roots of political life and the importance of local and regional research, moreover, there is no attempt in the book itself to deliver on this promise. The claim to be building a bridge between high politics and "locally oriented *Alltagsgeschichte*" bears no relation to the discussion's actual contents.

In brief, this is not a significant addition to the literature on the *Kaiserreich*. It provides a passing encounter with a scattering of recent research. But, for a fresh and challenging introduction to the social history of politics under the empire, people should go elsewhere.

GEOFF ELEY
University of Michigan

KLAUS ERICH POLLMANN. *Parlamentarismus im Norddeutschen Bund 1867–1870*. (Handbuch der Geschichte des Deutschen Parlamentarismus.) Düsseldorf: Droste. 1985. Pp. 598. DM 138.

Except for those eager to chronicle Bismarck's diplomatic artistry, German historians have not been much concerned with the years 1866–70. This is unfortunate because the period of the North German Confederation contained developments of lasting significance for Germany's future. Exposed for the first time to the dangers and opportunities of democratic suffrage, the old alignment of political forces began to shift, break, and reassemble. Faced with the need to create a national social and economic system, liberal parliamentarians were able to pass a series of fundamentally important laws on trade procedures, currency regulations, and commercial practice. Finally, by ratifying and then testing the constitutional limits of their power, the members of the North German Reichstags helped set the trajectory for the development of parliamentary institutions throughout the Bismarckian era.

We learn a great deal about the North German Confederation's political life from Klaus Erich Pollmann's contribution to the multivolume Handbuch der Geschichte des deutschen Parlamentarismus, edited by Gerhard A. Ritter. Pollmann provides a useful guide to the formulation of the new suffrage law, analyzes the elections of February 1867 (to the constituent Reichstag) and August 1867 (to the North German Reichstag), traces the most important debates in both bodies, and examines with particular care those matters having to do with the parliament's role in the political system, such as electoral laws, budgetary rights, and regulations on ministerial responsibility. From his account, two themes emerge as especially compelling. First, we are struck by the persistent influence of foreign affairs on both the government and the Reichstag. Bismarck and his liberal allies were continually aware of the need to consolidate the nation, protect it from prospective enemies abroad, and attract potential allies among the states south of the Main. As he was to do so often in the years ahead, the chancellor was able to use these foreign political fears and ambitions with great skill. Second, we are impressed by how clearly the strengths and weaknesses of German parliamentarism are foreshadowed during this period. In Pollmann's words, "the successful legislation found its limits when it no longer concerned the reform of relatively autonomous social spheres but touched the political authority of the state's power apparatus" (p. 456). Despite its formidable accomplishments in the realm of social and economic policy, therefore, the Reichstag's political authority was severely limited. Parliamentarians were never allowed to forget completely that the confederation was, as Bismarck's spokesman Hermann Wagener put it, "not the child of constitutional parents."

Pollmann's book may have suffered from the restraints imposed by the series of which it is a part. Because he must cover a certain number of issues, he is not always able to develop his own hypotheses and sometimes has trouble constructing an overarching explanatory structure. Moreover, it seemed to me that Pollmann paid too much attention to Prussia and not enough to the territories newly annexed in 1866 or to the other states in the confederation. I would like to have learned more about how the

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North German parliament affected political alignments and institutions in these areas. Nevertheless, Pollmann has done us a great service with this carefully researched and clearly argued book, which will, it is to be hoped, inspire more scholarly work on a decisive era in German political development.

JAMES J. SHEEHAN
Stanford University

STANLEY SUVAL. *Electoral Politics in Wilhelmine Germany*. Chapel Hill: University of North Carolina Press. 1985. Pp. xi, 311. $30.00.

Conventional wisdom has it that the Germans were an "unpolitical" people before 1914, unused to the exercise of democratic rights and burdened with a long tradition of political quiescence. The legacy of the "unpolitical German," indeed, has been made widely responsible for many of the failures of Weimar democracy. Stanley Suval's absorbing new book now reveals this, like so many other facets of conventional wisdom about the Germans, to be a myth. Using concepts and techniques developed in American electoral analysis, Suval shows that voting in Wilhelmine Germany, especially after the mass politicization of the 1890s, was a means of demonstrating social and political commitment. This led to increasing demands for electoral reform in order to increase the direct political effects of elections. Despite the prominence of government-inspired issues in elections such as that of 1907, Wilhelmine Germany was not a "plebiscitary" system in which elections merely served the function of legitimizing an irremoveable government. People voted to affirm their allegiance to a particular class, religion, or ethnic grouping. Elections were fair, orderly, and remarkably free from corruption and intimidation. Electors voted habitually, sustained their commitment in run-off elections, and regarded elections as important and meaningful political events. In all these respects, Suval shows, elections and electors in Wilhelmine Germany were comparable with their counterparts in Britain and America. The electoral problems of the Weimar Republic derived not from its Wilhelmine inheritance but from the disruption of that inheritance, above all in the massive crisis of 1929–32. As Suval argues, such a crisis might undermine the political stability of any capitalist system.

These are convincing arguments. They are buttressed by a range of statistical analyses of turnout, voter preference, and socioeconomic indicators and by a useful set of case studies of different electoral districts. The book also provides much-needed basic information on the Wilhelmine electoral system in general. Suval's idea of applying the tools of contemporary electoral analysis to Wilhelmine Germany is so obvious that one wonders why it has not been done before. It is unfortunate, therefore, that the book is not always well written and that it is marred by clumsy translations of German quotations and considerable carelessness of detail (such as the misspelling of *Sanitätsrat*, the description of typhus, whose vector is the human bodylouse, as an airborne disease [both on page 210], and the numerous minor errors that riddle the bibliography). And Suval commits a real howler when he quotes the Junker Elard von Oldenburg-Januschau as declaring that the king of Prussia should be able to say at any time to an army lieutenant, "Take ten men and shoot the Reichstag" (p. 194). Oldenburg-Januschau was certainly a combative individual, but he was not a madman; what he actually said was not "shoot" (*schiessen*) but "shut" (*schliessen*) the Reichstag. This makes one wonder what other errors of note-taking a closer scrutiny might reveal.

Nevertheless, these criticisms should not be allowed to obscure the fact that this is an original and thought-provoking book that makes a major contribution to the current rewriting of late nineteenth- and early twentieth-century German history. It is certainly not the last word on the subject: we badly need in-depth studies of individual elections, and the book's general orientation toward Reichstag politics leaves room for much more work at the level of the federated states and municipalities. But Suval has written a book that nobody interested in Wilhelmine Germany can afford to ignore, and everyone who reads it will do so with profit.

RICHARD J. EVANS
University of East Anglia

MICHAEL BEHNEN. *Rüstung, Bündnis, Sicherheit: Dreibund und informeller Imperialismus, 1900–1908*. (Bibliothek des Deutschen Historischen Instituts in Rom, number 60.) Tübingen: Max Niemeyer. 1985. Pp. xi, 516.

The Triple Alliance of Austria-Hungary, Germany, and Italy was formed in 1882 and ended thirty-three years later in May 1915. The weaknesses of this alliance were evident from the very beginning because of Italian irredentism and the conflicting interests of Austria-Hungary and Italy on the Adriatic Sea and in the western Balkans. Yet the alliance survived Italy's entente with France in 1902 and the Racconiggi agreement with Russia in 1909, both of which opened the way for Italian cooperation in the Mediterranean and Balkans with the enemies of Germany and Austria-Hungary. To explain the durability of the Triple Alliance, historians have emphasized that it was a *Vernunftehe* that permitted Austria-Hungary and Italy to watch over each other and to avoid direct confrontations that

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could lead to general European war. The alliance has, therefore, often been seen as a stabilizing factor that helped regulate relations among the powers and preserve peace for many years in Central and Southern Europe.

Michael Behnen faults this traditional view of the Triple Alliance for accepting the *Primat der Aussenpolitik* and neglecting the influence on foreign policy of such factors as national social and economic policy, internal political and constitutional structures, national armaments programs, and the financial and industrial relations of the great powers with each other as well as with the smaller, weaker nations of Southern and Eastern Europe. Behnen bases his book on impressively thorough and exhaustive research in diplomatic, financial, commercial corporate, railroad, administrative, and newspaper archives located in Bonn, Koblenz, Freiburg, Essen, Potsdam, Vienna, Vincennes, Paris, and Rome.

He makes particularly effective use of the new evidence he has found in these archives to describe in detail the nature of French and German armaments exports and the competing and interacting financial and industrial penetration of Italy and Romania. He shows how such competition and penetration combined with Austrian and Italian commercial and industrial imperialism on the Adriatic and in the western Balkans to make the Triple Alliance an ineffective instrument for the maintenance of peace in Central and Southern Europe. On the contrary, as Behnen points out, the weaknesses of the Triple Alliance would seem to have drawn Germany and France into a competition to strengthen the economies and military potential of unstable governments in Bucharest, Rome, and Vienna, whose imperial aspirations and war plans represented a serious threat to the general peace and security of Europe.

Ending his account in 1908, Behnen is not able to provide a fully developed, new interpretaion to replace the brief, standard survey of the history of the Triple Alliance published by Fritz Fellner some twenty-five years ago. In the introduction to his book Behnen mentions that a Jens Petersen is preparing a similar study for the period 1909–14/15. Such studies are extremely important and offer materials and a conceptual framework for rewriting the diplomatic history of Europe during the period preceding the outbreak of World War I.

EDWARD C. THADEN
University of Illinois, Chicago

HORST MÖLLER. *Parlamentarismus in Preussen 1919–1932.* (Handbuch der Geschichte des Deutschen Parlamentarismus.) Düsseldorf: Droste. 1985. Pp. 662. DM 148.

The Commission for the History of Parliamentarianism and Political Parties continues to turn out one volume after another, all scholarly and well documented but not all of equal value. Horst Möller, the author of this "handbook," has gathered together under one cover a mass of material concerned in one way or another with the parliamentary system of the state of Prussia in the years of the Weimar Republic. The early chapters describe the drawing up of the Prussian constitution and compare that document with the national Weimar Constitution. Much of the rest of the book is a detailed and often tedious analysis of the implementation of the constitution: the system of state elections, the legislative process, the composition of the political party delegations and the ministries, and the relationship of the state government to the civil service. All of this provides a useful supplement to similar analyses already made of the national government. Other portions of the book, however, deal with the political narrative, particularly those describing ministerial crises and the abrupt end of parliamentary government in Prussia, when Franz von Papen imposed a Reich commissioner in 1932. Since Prussian and Reich politics were closely interrelated, this involves considerable digression into German politics and personalities at the national level. Considering the turbulent nature of the Weimar years, this admixture is perhaps inevitable, but it is oddly disconcerting to the reader, as if sensational newspaper clippings had been inserted into the pages of a political science textbook. There is too much disparate material here to be covered adequately in one book, yet not enough for two. The analytical content would be more meaningful if it were fleshed out with more information about Prussia in that period: its demographic make-up, its economy, and its regional variations (for example, the *Land* provinces should be listed and described). The political content deserves to be given more attention and to be presented chronologically rather than to be inserted as a series of examples of parliamentary function.

This said, it should be noted that Möller does present an interesting thesis. He believes that the case of Prussia "casts new light" (p. 579) on the familiar theory that Germany's constitutional history followed a special path (*Sonderweg*), different from the development of parliamentary government in Western Europe. Möller points out that the Prussian constitution of 1920 contained none of those structural flaws in the Weimar Constitution that are so often cited as contributing to the downfall of the republic, and he reveals that the Prussian system in fact closely resembled those of the Western democracies.

Most important, the Prussian constitution did not provide for a head of state analogous to the Weimar

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president, in spite of strenuous efforts to include one on the grounds that it was necessary to prevent "parliamentary absolutism." Instead, the weight of authority was placed in the *Landtag* itself, which had the sole responsibility for choosing the minister-president and also the ultimate responsibility for resolving party conflicts and for issuing emergency orders, powers that could be and were taken over by the supposedly "independent" Reich president at the cost of weakening the Reichstag. The legacy of the Prussian system has been that, whereas "Bonn ist nicht Weimar," the present German Federal Republic is structured very similarly to the "Prussian alternative" model (pp. 598–99).

Unfortunately, Möller's description of the smooth and democratic operation of the Prussian system, which continued until it was destroyed by "outside forces," fails to consider that Papen's intervention in 1932 could not have occurred if the voters of Prussia, like those in Germany as a whole, had not already elected a *Landtag* majority of extremist delegates openly committed to the destruction of the parliamentary regime. Until this point, it is true, Prussian elections had produced a much more stable alignment of parties, maintaining the Weimar Coalition in office for most of the republican years. This greater stability resulted, as Möller explains, from the influence of the Catholic Center party, which remained undivided and moderate in the state; at the national level, meanwhile, the secession of the reactionary, antirepublican Bavarian People's party had a destabilizing effect. But by the 1930s this formerly reliable Prussian electorate had been affected by the national tendencies, resulting in the shrinking of the liberal middle and the polarizing growth of the Communist and National Socialist parties. Whatever the perfections of their constitutional structure, democracies cannot survive without voters who believe in them. Prussia's brand of parliamentarianism did not long outlast the Reich's.

ELLEN L. EVANS
Georgia State University

PETER KRÜGER. *Die Aussenpolitik der Republik von Weimar*. Darmstadt: Wissenschaftliche. 1985. Pp. xiv, 605.

The problems of continuity in German history have never been simple, and they promise to get more complicated as the German Federal Republic grows older. Twice in this century Germany has had the task of restoring its international status after a lost war. It is now quite clear that its best chances for doing so and surviving lay in a basic acceptance of military defeat and in accommodation and collaboration with the victors, especially the Western powers. The restoration of Germany's status as a major power and of Germany's domestic tranquility depended on an economic recovery based on the country's promise as a trading nation. The successful pursuit of such policies, which also seem to have stilled irredentist and revanchist sentiments, appears as one of the novelties of the Federal Republic and one of the most telling illustrations of why Bonn is not Weimar. The brevity of the Weimar Republic, the tarnishing of Gustav Stresemann's reputation by some of the damaging material found in his papers, and the horrendous policies pursued by the National Socialists have all tended to support those who would stress the continuity of German imperialism. Historians more favorable to Stresemann may have spoken of his foreign policy but not of a Weimar foreign policy. In 1958, Ludwig Zimmermann, the last historian to have ventured a full-scale survey, entitled his study *"German"* foreign policy in the Weimar era and blamed reparations for the collapse of Weimar and of Europe's peace. As Peter Krüger demonstrates in his important reinterpretation, however, Weimar foreign policy differed from those of the belligerent regimes that preceded and followed it and was in many important respects a predecessor to the foreign policy of Bonn. These arguments, as much as the author's exceptional command of the primary sources and the secondary literature, make this book a work of major significance for both German history and the history of international relations.

Krüger has not written a work either of apology or of illusion, and he avoids the trap of permitting his readers an excess of fantasy about the long-term chances of Weimar and its foreign policy. The latter, like so much of Weimar, had its domestic foundation and origins in the coalition that made the peace resolution of July 1917 and depended on the willingness of the more moderate forces on the political Right for its success. Its basic principles were the peaceful mediation of international differences, a liberal foreign trade policy, and a new style of diplomacy designed to give other nations confidence in Germany's reliability and credibility. The shock of the Treaty of Versailles and the domestic instability of the republic's first years made the establishment of this policy difficult. Moreover, much of early Weimar's foreign policy originated outside the Foreign Office (then undergoing reorganization), which made matters worse.

One of the major contributions of Krüger's book is to demonstrate that the success of Weimar's foreign policy during the republic's middle years was owing not only to Stresemann's brilliant leadership but also to the key persons at the top of the reformed Foreign Office, especially the remarkable State Secretary Carl von Schubert, who appears to have defined the basic ideas of the new German diplomacy, along with his able and supportive col-

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leagues Ago von Maltzan and Karl Ritter. Although Krüger certainly is very sensitive to the domestic political and international economic foundations of German foreign policy from the Dawes Plan through Locarno to the Young Plan, his account is written from the perspective of the Foreign Office, which briefly appears as a somewhat untroubled island in a calm sea.

As Krüger shows, however, the calm was deceptive, and time was not on the side of Weimar. Germany's gains were insufficient to satisfy an important portion of public opinion or to pave the way for the kind of gradual acceptance of the Versailles territorial settlement that Schubert appeared to favor. American economic protectionism did not help either. Most important, the turn to the right in German domestic politics that accompanied the onset of the depression undermined Weimar foreign policy and replaced it with the whining, strident, and incalculable foreign policy of Heinrich Brüning and of Schubert's successor at the Foreign Office in 1930, Bernhard Wilhelm von Bülow. Krüger's description of the change of tone, style, and content of German foreign policy in 1930 is a tour de force of historical exposition and strengthens the case for those who would assert that the demise of Weimar democracy took place in March 1930. It is a brilliant ending to what certainly must be considered the basic work on Weimar foreign policy and the touchstone for all future discussion and research on this subject.

GERALD D. FELDMAN
University of California, Berkeley

WILLIAM L. PATCH, JR. *Christian Trade Unions in the Weimar Republic, 1918–1933: The Failure of "Corporate Pluralism."* (Yale Historical Publications, Miscellany, number 133.) New Haven: Yale University Press. 1985. Pp. xvii, 259. $22.50.

The traditional emphasis in German trade union history was on the Socialist (or "free") trade unions to the exclusion of other significant organizations. In the past decade this has changed with the appearance of studies of the Christian trade unions by Rudolf Brack, Hans Dieter Denk, and, especially, Michael Schneider, who produced a tome dealing with the history of Christian labor from its nineteenth-century origins until its demise in 1933. Based on painstaking research in fourteen archives, William L. Patch's book on the Christian trade unions in the Weimar Republic is a welcome addition to this new body of literature. Patch not only delves into aspects of the movement glossed over or overlooked by Schneider, such as parliamentary politics and the activity of white-collar unions, but also offers a more positive assessment of the historical significance of the Christian organizations.

The study focuses on Christian labor's frustrating attempts to achieve a cooperative relationship between capital and labor that could serve as the basis for responsible social and economic policy, parliamentary stability, and a future for democracy. The quest to make the *Arbeitsgemeinschaft* of the immediate postwar years a functioning reality faltered, as did efforts to advance social legislation in the mid-1920s or to achieve agreement on depression-relief programs, on the tendency in middle- and upper-class circles to hold workers responsible for the unacceptable outcome of the war and the disruption of orderly life in general. Patch offers ample documentation that Christian initiatives were further complicated by the Socialist unions' intransigence, irresponsibility, and unwillingness to depart significantly from prewar patterns of struggle. To cite one example, Karl Legien, national chairman of the Socialist unions, complained bitterly to his colleagues "that none of them displayed the energy and dedication with which the leaders of the Christian unions hurled themselves into the task of administering the [*Arbeitsgemeinschaft*]" (p. 37).

Here Patch disagrees vehemently with Schneider. That the Christian trade unions were programmatically linked to bourgeois groups, that they passionately advocated monarchy, nationalism, and imperialism, and that they were reserved in support of the Weimar Republic and eventually offered to participate in building Hitler's Reich make it difficult, Schneider concludes, to see how today's workers could relate to, or build on, the tradition of the Christian trade unions of the prefascist era. Patch argues that conciliation, compromise, and cooperation—not struggle—were what Weimar needed and that the largely strife-free relationship between capital and labor in West Germany since 1945 confirms the realism of Christian labor's approach. Patch's rejoinders definitely weaken Schneider's case, but, as in most scholarly debates, the "truth" lies somewhere between the poles. Caught up in this disagreement of near-polemical proportions, Patch misses an opportunity to go beyond antithesis and offer us synthesis, or at least to allot more space to the major contribution of the book: the analysis of the extraordinary degree of antiworker sentiment in the phobia-ridden middle and upper classes of Weimar.

In my opinion Patch also misinterprets the motivation behind Christian social reform. The emphasis on conciliation, arbitration, and codetermination was not secular; rather, it stemmed from the neo-Thomist belief that access to the benefits of private property should be widely shared in the community. The propagation of codeterminist programs in particular was a hallmark of the Christian labor move-

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ment and was not, as Patch states, borrowed from the Social Democrats (pp. 124, 230).

But these criticisms and disagreements in no way diminish the major contribution that Patch makes. His book is a solid, well-written monograph that represents empirical history at its best. All of us in the field need to give it a careful read.

ERIC DORN BROSE
Drexel University

DIETER RIESENBERGER. *Geschichte der Friedensbewegung in Deutschland: Von den Anfängen bis 1933*. (Sammlung Vandenhoeck.) Göttingen: Vandenhoeck und Ruprecht. 1985. Pp. 297. DM 39.

Publication of this book is timely. It serves in the first place to remind activists in today's West German peace movement that their campaign represents the continuation of an important tradition in German history. A valuable introduction to this historical tradition, the book also serves as a survey and synthesis of recent monographic literature on the history of the German peace movement—most important, the work of Friedrich-Karl Scheer, Reinhold Lütgemeier-Davin, Helmut Donat, Wilfried Eisenbeiss, Karl Holl, Dieter Riesenberger himself, and my own.

Those who are familiar with this literature will find few surprises in Riesenberger's account. Throughout the period under consideration, the peace movement remained in most respects a foreign body in a political culture fashioned by the acceptance of war as an inevitable or positive feature in international affairs. Until 1914 the movement was the preserve of a small group of middle-class Progressives, who advocated international arbitration and arms limitation. The object of official harassment and persecution during the war, the movement reached the apex of its development and appeal in the aftermath of war, when advocacy of international organization exerted a certain patriotic attraction as a means of gaining a compromise peace and then of erasing the injustices of the Versailles settlement. Nonetheless, although it was much larger, its social composition more diverse, and its program much more the subject of public controversy than before the war, the peace movement was scarcely less impotent during the republic than it had been under the empire.

The strength of Riesenberger's book is the clarity of its narrative of organizational and programmatic developments within the German peace movement. The book also presents useful synopses of the peace movement's relations to the political parties (most critically, to the Social Democratic party), the government, and the churches. The most original part of the book is a stimulating chapter on the contrasting portrayals of the experience of war that emerged in the pacifist and nationalist literature of the 1920s. Owing, no doubt, to limits imposed by the publisher, the author has left undeveloped some of the broader problems that his narrative raises. Before 1933, opposition to war was clearly not a cause capable of generating any kind of idealogical consensus in Germany, even among those who espoused the cause. Pacifism comprehended, in other words, conflicting visions of society. If the extreme marginality of the peace movement hid this fact before the war, the expansion of the movement in the 1920s brought staid middle-class democrats together with left-wing socialists, whose "radical pacifism" was revolutionary and hence repugnant to the men who had earlier been the movement's leading element. The radicals' program called for draft resistance, general strike, and, ultimately, for the end of capitalism as the only effective solution to the problem of war. The disputes that raged within the peace movement in the 1920s and led in the end to its organizational fragmentation thus had to do with far more than disagreements over tactics; they reflected the fragmentation of German society itself.

ROGER CHICKERING
University of Oregon

ALMUTH MEYER-ZOLLITSCH. *National-Sozialismus und Evangelische Kirche in Bremen*. (Veröffentlichungen aus dem Staatsarchiv der Freien Hansestadt Bremen, number 51.) Bremen: The Archive. 1985. Pp. 388.

The historiography of the German church struggle has gone through four discernible phases since 1945: the eyewitness accounts and memoirs of active participants, recording opposition to the encroachments and persecutions suffered at the hands of the Nazis; compendious studies by church historians justifying the conduct of the Confessing Church and attacking the policies of its pro-Nazi opponents, the "German Christians"; accounts of particular local church situations; and, more recently, surveys with a broader perspective, which place the church struggle in its context as part of the whole Nazi experience. Almuth Meyer-Zollitsch's thoroughly researched study of the conditions in Bremen nicely weaves the story of one such local community into the wider picture and seeks to portray the whole spectrum of Protestant attitudes within the setting of the Nazi bid for totalitarian control.

Bremen had a long history of republican, bourgeois, and independent leadership. It was one of the cities least susceptible to the Nazi allurements, and its church life was dominated by a vigorous liberalism and local autonomy. The Nazi takeover of power was successful only because of the economic

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depression and the consequent lack of confidence in the ruling circles. The "German Christian" attempt to seize control of the ecclesiastical structures was undertaken by the flamboyant figure of Heinrich Weidemann, a determined careerist, whose sermons were preached "with the Bible in one hand and a grenade in the other." He opportunistically joined the Nazi bandwagon in 1933 and used his connections with the Nazi leadership to have himself proclaimed bishop of Bremen. His arbitrary and dictatorial style of church government, his overenthusiastic endorsement of Adolf Hitler as the messiah of the new Germany, and his perversion of the gospel in favor of the Nazi racist ideology soon aroused fierce opposition. But his eventual downfall in 1942 resulted not from these excesses but from his scandalous divorce, his adultery, and his conviction for perjury, which finally led to his abandonment by the Nazi hierarchy.

At the other extreme was the pastor of St. Stephen's Church, a rigorist "evangelical" whose biblically based witness refused to allow any compromises with the Nazis or with the moderate "middle party." Inevitably, in such a small society of only fifty-one pastors, personal antagonisms exacerbated the theological divisions and severely hampered the efforts to unite all the churches against Weidemann's increasingly wayward tactics of expulsions, expropriations, and denunciations.

Meyer-Zollitsch's sympathies are clearly with the moderate liberals who sought to preserve Bremen's tradition of tolerance in church affairs. Their leader was a committed layman, Karl Stoevesandt, whose personal account, *Bekennende Gemeinde und deutschgläubige Bischofsdiktatur* (1961), is one of the best of its kind. She depicts the fatal schizophrenia of this middle group, torn between their desire to resist Nazi oppression in the church and their wish to affirm the Nazi state on all other grounds. And she rightly points out how this ambivalence prevented the mobilization of more effective protests against the worst excesses of the Nazi regime, for example, against the deportation of the few remaining Bremen Jews. Only St. Stephen's Church held a farewell service for its departing Jewish-Christian members, for which the pastor was banished from Bremen for the remainder of the war, having been denounced by a fellow clergyman.

In the concluding chapter Meyer-Zollitsch ably summarizes the reluctant acceptance of the lessons of the church struggle in the Bremen churches, which, like their counterparts in the rest of the country, only slowly and unwillingly came to realize that the silence of the churches toward the victims of Nazi misrule had resulted in such fateful consequences.

JOHN S. CONWAY
University of British Columbia

HELMUT FIEDERER. *Reichswerke "Hermann Göring" in Österreich (1938–1945)*. (Veröffentlichungen des Historischen Instituts der Universität Salzburg, number 16.) Vienna: Geyer. 1983. Pp. 303.

Hitler's "racial" and political reasons for annexing Austria are well known. Much less familiar are the economic motivations for the Anschluss and the benefits it had for the Third Reich. Helmut Fiederer attempts to answer these questions as well as to provide some insights into the importance of the *Ostmark* for Nazi Germany's war machine.

Fiederer maintains that the addition of the *Reichswerke "Hermann Göring"* helped give the party a decisive edge in its struggle for supremacy with the old industrial elite and the Wehrmacht. The factories played an important role in Göring's personal empire and in his competition with Heinrich Himmler and the SS. In addition, the Austrian enterprises, which were founded or modernized by Göring in 1937, furthered Germany's economic penetration of Southeastern Europe by providing less developed countries with expensive finished products in exchange for cheap but scarce natural resources.

The Anschluss came at a most propitious time in the economic history of the Third Reich. Germany was rapidly developing a critical labor shortage, lacked sufficient steel for consumer goods because of rearmament, and could supply only 18 percent of its own iron ore needs. The annexation of Austria temporarily eased all of these problems, although it did not provide a permanent solution to any of them. Austria, which still had four hundred thousand unemployed workers in 1938, had huge reserves of iron ore, and Göring's new smelting plant in Linz became an important producer of steel. Even though it never reached full capacity, the *Eisenwerk Oberdonau* became the biggest producer of parts for heavy tanks in Greater Germany.

The new and modernized heavy *Reichswerk* industries of Austria remained relatively immune from Allied bombing. While industrial production was falling by 20 to 58 percent in other parts of the Reich between May 1944 and January 1945, it fell by only 9 percent in the Danubian and Alpine *Gaue*, thus earning for Austria the title of *Luftschutzkeller des Reiches* (p. 221).

Fiederer has ably exploited several business and private archives as well as relevant secondary literature. His book contains many useful charts and diagrams as well as copious notes, a name (but not a subject) index, and a few illustrations. The book suffers, however, from too narrow a focus, and the author makes numerous undocumented assertions. We are told, for example, that the Alpin-Montan mining company at Donawitz was modern in 1914 but relatively antiquated by 1938. Working conditions finally improved there after a strike in 1940,

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but we are not told how this was accomplished. Likewise, we have to accept on faith that the nationalized *Reichswerke* in Linz, renamed the United Austrian Iron and Steelworks, played a critical role in the economic history of the Second Austrian Republic.

In sum, this book will appeal to only a limited number of specialists interested in the business history of the Third Reich and the economic modernization of Austria.

BRUCE F. PAULEY
University of Central Florida

JOHN E. FARQUHARSON. *The Western Allies and the Politics of Food: Agrarian Management in Postwar Germany.* Dover, N.H.: Berg. 1985. Pp. 279. $31.95.

In the past year, two remarkable academic studies have appeared on food-supply management in Britain's post–World War II occupational zone of Germany: Gabriele Stüber's exhaustive *Der Kampf gegen den Hunger 1945–1950* and John E. Farquharson's study. Although covering the same basic time period and materials, the emphasis of each is very different. Stüber stresses the evolution of British occupation policies, whereas Farquharson largely emphasizes Anglo-American relations in the agrarian sector and their implications for Germany and postwar Europe. His major theme is how Britain, saddled with heightening responsibilities but possessing inadequate resources, had to appease the U.S. in order to solve British problems. Secondary themes are the connections between the British control of food and the reconstruction of Europe and the timing of the bizonial fusion and its later implications for West Germany, especially in terms of the degree of socialism accomplished in the British zone.

The British faced some catastrophic problems. Their zone, the most populous and industrialized of the four, was now swollen by a wave of refugees, cut off from food-exporting areas in the east, hampered by the breakdown of central administration, and confronted with a sharply reduced production of food. The British were thrown on their own meager resources, much to the displeasure of the government and the British taxpayer. Serious food shortages quickly developed and, despite the best efforts of the British, persisted until 1949. At the same time, Britain's allies were demanding increased mining and industrial production to help save the rest of Europe. All of this played against a background of the growing cold war. Squeezed between their home and imperial responsibilities on one side and their obligations in Germany on the other, the British had no choice but to appease the Americans and merge their zone as Washington was suggesting. Within the zone this meant compromising their position on the socialization of German heavy industry, a goal favored by the Labour government. The implications for the future Federal Republic are obvious, but Farquharson's arguments are debatable, for there was already a strong undercurrent of opposition, among Germans as well as Americans, to socializing industry. Regardless of their plans, the British were in no position to confront their stronger ally in Germany and had to accede to the Americans. With new, huge postwar commitments and a declining resource base, the British simply could not afford to accept the further burden of a devastated zone in Germany. The British were in a hopeless situation. To comply with the Potsdam agreements meant feeding their zone without transfers from the east and that meant imports from abroad—paid for either in reduced rations at home or from precious foreign exchange. The British were pushed into dependence on the U.S.

This is a fair and sympathetic account of the British attempts to solve the prolonged food crisis. It is well researched and written and compliments Stüber's work.

EDWARD L. HOMZE
University of Nebraska

WOLFGANG BENZ. *Von der Besatzungsherrschaft zur Bundesrepublik: Stationen einer Staatsgründung 1946–1949.* Frankfurt a/Main: Fischer. 1984. Pp. 319. DM 14.80.

DIETER MEIN. *Zwischen liberaler Milieupartei und nationaler Sammlungsbewegung: Gründung, Entwicklung und Strukur der Freien Demokratischen Partei 1945–1949.* (Beiträge zur Geschichte des Parlamentarismus und der Politischen Parteien, number 76.) Düsseldorf: Droste. 1985. Pp. 402. DM 68.

Wolfgang Benz and Dieter Mein belong to a generation of bright young German scholars who have been writing up a storm about the immediate postwar history of Germany. The editor of a three-volume history, *Die Bundesrepublik Deutschland* (1983), Benz has also written a more conventional survey of the same topic, *Die Gründung der Bundesrepublik: Von der Bizone zum Souveränen Staat* (1984), and is involved in the first book of the forthcoming authoritative five-volume *Geschichte der Bundesrepublik.* Many of the German scholars studying recent postwar history have been associated with the Munich Institut für Zeitgeschichte or have received the impetus for their research from there, but it would be more correct to say that the word has been pouring forth from all over West Germany.

Benz's book about the formative period of the Federal Republic particularly stresses the part that the Germans themselves played in the reconstruc-

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tion of government at all levels (the Allies' role will be discussed in a companion volume by Hermann Graml). Benz, by now a veteran of German postwar studies, painstakingly analyzes the various levels of action and institutions among the Germans and relates them to one another. Striking glimpses of personal encounters among the principals and of the initially grim social realities illuminate the scenarios of rapid political change. The author's emphasis on institutional politics is lightened with personal portraits of the leading figures and their respective preferences for different paths of West German development.

The resulting picture of the evolution of these fateful years is rather complete, and yet it leaves the reader wishing for broad and meaningful concepts and models that would help order the luxuriant thicket of particulars in the reader's mind. Looking over the vast recent output on this subject by other German authors and, even more important, over the growing literature on comparable transitions from fascist dictatorship to postwar democracies in other European countries increases the thirst for meaningful generalizations. The case of postwar West Germany was not really so unique that it could not be compared in important ways to Austria or Italy or even to the transition from collaborationist or crypto-fascist regimes in various Nazi-occupied countries to democracy. Comparison, for example, can generate a periodization related to the hour of liberation and to the purges of this moment and can reveal in an instant the extraordinary exception that the Germans were not permitted to botch their own de-Nazification, or botch it in their own way, because the Allies each insisted on doing it for them. Comparison also focuses automatically on the organization-forming or reforming periods in each of these countries, the reshaping of their party and interest group systems, and their institutional reconstruction and constitution-writing phases—the focus of much of Benz's description—thus giving a temporal goal to each country's process of transformation. One could even compare the step-by-step process by which the Western Allies gradually advanced the West Germans toward self-government—over the decade 1945–55—to that used by the British to turn former colonies into self-governing commonwealths.

Speaking of the organization-forming period in postwar Germany also brings us to Mein's exemplary study of the postwar reorganization of political liberalism after twenty years of crisis and massive defections to National Socialism. Mein's book is one of a vast series of dissertations and monographs published by the Bonn Kommission für Geschichte des Parlamentarismus und der politischen Parteien over three decades, a treasury of secondary works on the history of German parties. Mein seeks to overcome the simplistic view that the emerging postwar liberal forces were simply the heirs of the Weimar left-liberal Democrats and right-wing People's party and merely had to choose between these models or decide on an even-handed merger of the two. Instead, as he demonstrates region by region, there were far more options available, and the outcome depended on the influence of such major factors as religious composition, regional social structure, and surviving liberal traditions—especially of the left-liberal variety—and on prominent liberal Weimar leaders who were often appointed to important postwar offices by the occupation. A strong reservoir of liberal voters and leaders was no guarantee against factional infighting during the reformation of political liberation, but it held at bay the threat of mergers with the Christian Democrats, the attempts of a Berlin group to reestablish a liberal party from the top down, and the menace of the competing liberal ideal of a nationalistic, mass-mobilizing *Sammelbewegung* to the right of all other established parties. The latter had its advocates in regions like Hesse, North Rhine Westphalia, and Lower Saxony and experienced its heyday during the crisis years of the major parties, 1948–53, when radical right-wing movements made major inroads on the Free Democratic party (FDP). Instead, as Mein explains succinctly, the left-liberal *Milieupartei* between the Social Democratic party and the Christian Democratic Union won out and became the pattern that has characterized the role of the FDP in the West German party system to this day.

PETER H. MERKL
University of California, Santa Barbara

ECKHARD JESSE. *Wahlrecht zwischen Kontinuität und Reform: Eine Analyse der Wahlsystemdiskussion und der Wahlrechtsänderungen in der Bundesrepublik Deutschland 1949–1983*. (Beiträge zur Geschichte des Parlamentarismus und der Politischen Parteien, number 78.) Düsseldorf: Droste. 1985. Pp. 440. DM 74.

A massive study of the West German electoral system, no matter its quality, will not hit the best-seller list in that country or any other. Indeed, that is the sad but inevitable fate for most scholarly books dealing with a specialized topic of interest to only a limited audience. Moreover, let's face it: electoral law studies are not exactly a growing field.

In this instance, Eckhard Jesse has revised and drastically cut his dissertation to produce a 440-page book of electoral law discussions and reforms in the Federal Republic. Its length attests to the thoroughness of the author in making use of available files in ministry archives, published materials, and electoral

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statistics. The result is a definitive work on the subject. Although the specialist will welcome the minute details of every reform and the pro-and-con discussions by most scholars who voiced an opinion on the subject, the general reader will wish that the editor had been firmer in making more judicious cuts in the manuscript.

Jesse first surveys the contributions made by scholars to the discussions on reforms of the post-1945 electoral system and then looks back at the proportional representation system of the Weimar Republic. He notes that few voices were raised at the time to provide more political stability by switching to a single-member plurality system on the British or United States model. In the post-1945 period many scholars blamed the failure of Weimar on a host of political, economic, and social causes rather than on proportional representation. But a minority of scholars, including Ferdinand Hermens, and Christian Democratic politicians viewed proportional representation, with its sprouting of minor parties, as the major cause for the political instability of Weimar governments. Jesse, in siding with the majority, contends that, if Weimar had not failed, then no one would have characterized the electoral system as a danger to democracy.

In the succeeding chapters the author examines the discussions revolving around the electoral laws of 1949 and their subsequent amendments in 1953, 1956, and 1970. He deals with numerous reform suggestions made by politicians and scholars in the 1950s and the 1960s and tests each one against a democratic yardstick. He then examines changes made in the electoral laws for the Bundestag after 1949: the controversial threshold clause, stipulating a 5 percent minimum of the vote or a minimum of three district seats for representation in the Bundestag; the two-ballot system, in which each voter casts one ballot for a candidate in the district (plurality system) and one ballot for the *Land* party list (proportional representation); the introduction of the absentee ballot; and the lowering of the voters' minimum age from 21 to 18.

Jesse supports all changes except for the two-ballot system. He criticizes the first ballot cast for the district candidates because a majority of voters do not know them, thus defeating the purpose of having a personalized election. In addition, the system enables the voters to split their two ballots among different parties, hence enabling the parties to manipulate elections, and confuses the bulk of the voters with its complexity. The author opts instead for a one-vote, modified proportional representation system in effect in the 1949 election, a system that was simple and democratic.

Although discussions about changes in the electoral laws have been less frequent in recent years, the rise of the Greens, who have surmounted the 5 percent threshold and want it changed or abolished, has renewed interest in the subject. The scholarly community should be grateful to the author for having provided a comprehensive survey of incremental changes made to this unique and complex electoral system.

GERARD BRAUNTHAL
University of Massachusetts

FREDERIC C. LANE and REINHOLD C. MUELLER. *Money and Banking in Medieval and Renaissance Venice.* Volume 1, *Coins and Moneys of Account.* Baltimore: Johns Hopkins University Press. 1985. Pp. xx, 684. $45.00.

Presented as the first volume of a monumental work on the monetary and financial history of late medieval and early modern Venice, this book has a far broader scope than it claims. Although Venice, one of the three major trade centers of Europe, is central to the volume, practically the book involves the entire European West, both as a paradigm and, through copious comparisons with other cities and states, as a point of reference. Indeed, the history of Venetian means of payment is imbedded and sometimes submerged in a general study of the economic and political thought of the period; in turn, late medieval and early modern thought and action are examined in light of our own theory and practice. The precise share of each of the two authors in the volume is not spelled out in a preface, but we do know that the late Frederic C. Lane was a master of Italian economic history and that he had been working on the subject for many years. We also know that Reinhold C. Mueller is a brilliant young scholar; that one cannot guess where his contribution begins is a good omen for a forthcoming second volume, for which he will have to bear full responsibility.

Studied at different levels by three separate categories of specialists—numismatists, historians, and economists—who do not get together often enough to pool their ideas and efforts, monetary history is seldom explored in all of its aspects at the same time. What makes this book especially valuable is its well-rounded and well-balanced approach. Here and there the exposition is rather diffuse but never unclear. A text of 491 pages in small print and 140 pages of appendixes, to say nothing of a full bibliography and index, cannot be summed up in the space of a standard review. Suffice it to say that the volume covers its ground thoroughly and that, on the whole, I have no strong objections to any of its statements.

That leaves me room for singling out a few points of mild disagreement with the authors. I think they overstress the differences between pawnbrokers, deposit bankers, and international moneylenders.

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Although I am quoted as one who "most recently accepted the division" (p. 70), I have constantly insisted that the differentiation hinged on the size of capital, which in turn determined the vocation. Most Jewish and Piedmontese pawnbrokers never attained a higher status, because they lent small sums to poor or reckless customers; hence, they earned little and were despised, although a few gathered enough capital by seizing defaulters' collateral and lost their bad odor. Indeed, all lenders in every class demanded a security and claimed it if the loan was not punctually returned. The international bankers of Genoa extorted from the weak king of Cyprus his best seaport, Famagusta; for some time they also pocketed a substantial part of Spain's fiscal revenues, but Spain was militarily powerful, and sooner or later the king defaulted scot-free. As for deposit bankers, they started a notch above pawnbrokers because they invested in their trade the money lent to them by their customers and corresponded to the customers an interest; in their trade, of course, they charged interest on loans. They could and did become international bankers if they amassed sufficient resources to go beyond local trade.

I note all this because the general title of the work promises a second volume on banking. But I am more alarmed by a short, unexpected excursus on bullionism in London, which is contrasted with a comparatively free flow of metallic currency in and out of Venice (pp. 65–68). Indisputably, monarchs were primarily eager to keep plentiful cash at home and to tap it for their expenses; indisputably, trading cities could not have traded without buying commodities for cash in one country and reselling them in another. Still, we now think that bullionism was economically damaging and hardly enforceable. Certainly the authors were well aware of this truism, yet the enthusiastic description of bullionism in London, followed by an apologetic mention of looser policies in Venice, might lead an unexperienced reader to side with bullionism. On the other hand, the authors' often-repeated statement that merchant cities were better advised than monarchies because the cities' monetary policies were determined by the governing merchants is certainly sound but might have been qualified by the remark that the merchants formed an oligarchy and did not necessarily pay attention to the wishes of the masses.

Last, I offer not a criticism but a friendly suggestion: when Mueller has completed the second volume and taken a well-deserved rest, could he be persuaded to write an abridged version of his (and Lane's) important work? Even for a fan of monetary history like myself, to read twice six hundred pages is a heavy load, competing, as it does, with so much other reading I ought to do. Much of this book can be condensed or encapsulated into footnotes. It would be a pity if so much knowledge were reserved for a minimal audience; one has to meet the larger audience halfway.

ROBERT S. LOPEZ
Yale University

HANS BARON. *Petrarch's* Secretum*: Its Making and Its Meaning*. (Medieval Academy Books, number 94.) Cambridge: Medieval Academy of America. 1985. Pp. ix, 254. $22.00.

Writing of Petrarch's *Secretum* in *From Petrarch to Leonardo Bruni* (1968), Hans Baron declared, "it [is] almost certain that the dialogue was composed between October 1342 and March of the following year" (p. 52). With minor variations, this dating was shared by most leading Petrarch scholars. Later, Francisco Rico argued with vast erudition that the earlier dating was based on Petrarch's fictional account of a dialogue with Saint Augustine (the content of the *Secretum*) and that the correct dating should be early drafts in 1347 and 1349, which Petrarch definitively reworked in 1353 (*Vida u obra de Petrarca*, vol. 1, *Lectura del "Secretum"* [1974]). Baron's new book, *Petrarch's* Secretum, is his response to Rico, whose later dating Baron accepts in its generality while wanting to demonstrate that the basic draft of 1347 was altered only in minor ways by insertions in 1349 and 1353. The use of revised dating to support new historical interpretation is a well-known feature of Baron's previous historical methodology. This new book is a polemic against the scholarship by which Rico supports his findings, and it is a retractation in part of some of Baron's earlier interpretations of Petrarch's historical role.

Six out of eight chapters are devoted to close examination of parallel Petrarchan passages that either confirm Rico's dating or, as Baron would have it, refute it. Other passages that should support Baron's modifications are also analyzed. Having examined Rico's presentation as well as Baron's objections, I conclude that both positions are arguable because an uncertain date is being established by the similarity of thought or language of passages in the *Secretum* and in other undatable writings of Petrarch. Rico's preference for 1353 and Baron's for 1347 support more general interpretations of the course and significance of Petrarch's career. Or so these authors believe. But is this so?

In his earlier studies of Petrarch, Baron argued against Umberto Bosco's conclusion that Petrarch so constantly revised his works that no dating of his ideas and attitudes was possible. Baron thought there was evidence of chronological layers that reflected changing positions in Petrarch's writings. Baron's scenario identified a bold break with "medieval" culture and religious convictions in Petrarch's early years, during which he pursued secular success and fame. The period climaxed in

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his coronation on the Roman Capitol for his achievements as poet, historian, and moral philosopher in 1341. This was closely followed by a great crisis of conscience expressed in the *Secretum* of 1342–43. In the dialogue Augustinus spoke for Petrarch's conscience on behalf of the pursuit of salvation, whereas Franciscus represented secular ambition, which won out in the end. A succeeding period of religious concern in the middle and late 1340s was succeeded by a return to classical values in the 1350s, which essentially continued until the end of Petrarch's life. So Baron summarized in *From Petrarch to Leonardo Bruni* (p. 49).

In this later study Baron seems ready to soften the contrasts between phases of Petrarch's career. The major religious writings (as Baron conceived them), the *De vita solitaria* and the *De otio religioso*, must have come before the *Secretum*, which registers no longer the crisis but the ongoing conflict of medieval religiosity and Renaissance secularism. He is more inclined to see Petrarch continuously supporting two contradictory approaches to life. He still appears to think that one set of interpretations is fitting for Petrarch as poet and another for Petrarch as historian and moral and political thinker.

Rico sees more consistency and continuity in Petrarch's roles. He also emphasizes the use of fictions, metaphors, and other poetic and literary devices in Petrarch's nonpoetical works. Curiously, Baron expresses moral outrage at this, claiming that Petrarch would not tell a lie and that the *Secretum* was a private confession for his eyes alone and not a work of literary and moral persuasion. Baron also believes that the notorious account in Petrarch's *Familiari* (17.5) of his ascent of Mount Ventoux, where he by chance opened Augustine's *Confessions* to a most appropriate passage, had to be literally true (pp. 196–202). This is a strange vestige of the medieval moralists' attacks on poetry because it lied.

Rico's views, which I find congenial, emphasize a higher moral seriousness on the part of Petrarch, who used the devices of poetry and rhetoric everywhere to move his readers to a new vision of a Christian life that was compatible with the late medieval/Renaissance historical world, one that combated the demoralization brought about by the excesses of secular ambition, something we have come to know as "lay piety." This vision was promoted by Petrarch in both 1347 and 1353. It does not matter if one or the other date marked the more definitive version. Rico does make some mistakes, and some of Baron's corrections are quite persuasive. More important is the difference in assessment of Petrarch's contribution to the emerging culture of humanism.

CHARLES TRINKAUS
EMERITUS
University of Michigan

RICHARD PALMER. *The* Studio *of Venice and Its Graduates in the Sixteenth Century*. (Contributi alla Storia dell'Università di Padova, number 12.) Padua: Lint. 1983. Pp. 204. L. 20,000.

Apparently a technical work for scholars of sixteenth-century medical history, this book should not be dismissed as solely that. For Richard Palmer has condensed into it years of archival work in Venice and Padua to make this a rich source of information on the intellectual life of sixteenth-century Venice. In addition, the book provides an understated but interesting report on the reconstitution of the archive of the *Studio* of Venice, destroyed when the college building burned down in January 1800.

First, however, it is a technical work that provides in appendix 2 (pp. 71–192) an annotated catalogue of graduates from the *Studio* of Venice (1501–1602) in medicine, philosophy, and surgery, along with a helpful index of names (pp. 193–204) that facilitates using the chronologically organized list. Extensive annotations make this list especially useful for those interested in intellectual history as well as those interested in the history of medicine.

A brief introductory essay puts the list in its historical perspective. Palmer argues that the *Studio*, though generally overlooked (in fact, some recent scholars have denied it existed), developed out of the desire of the already well established Venetian College of Physicians to grant degrees in the late fifteenth century. A formal papal bull of January 1470 establishing a *Studio Generale* in Venice confirmed an earlier privilege granted by the emperor in 1469, but the extensive powers granted—including the right to have faculties and grant degrees in theology, civil and canon law, arts, and medicine—were never realized. Rather, the *Studio* that evolved focused on medicine, surgery, and philosophy and even in those areas saw itself as ancillary to the more developed *Studio* of Padua.

Palmer holds that the Venetian *Studio* attracted students because it offered some noted professors, a more practical approach to medicine, and, especially important, degree prices that were substantially cheaper than those of Padua. He rejects, however, the idea that degrees were easier to win in Venice, pointing out that the formal exam procedure was similar in both *Studi*, that candidates who failed in Venice subsequently passed in Padua, and that an impressive number of Venetian graduates went on to have notable careers in medicine and philosophy. As one might imagine, competition from Venice was not always welcome by Padua, and Palmer briefly outlines a jurisdictional conflict at mid-century that resulted in lowered prices for degrees and a compromise designed to assure both institutions continued graduation fees. The essay concludes with an account of the destruction of the archive of the

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Studio and Palmer's tracing of its dispersed remains. These few pages modestly mask a major research project that one hopes is just beginning to bear fruit.

GUIDO RUGGIERO
University of Cincinnati

RAFFAELE FEOLA. *La monarchia amministrativa: Il sistema del contenzioso nelle Sicilie.* (Storia e Diritto, number 14.) Naples: Jovene. 1984. Pp. xvi, 334.

ARMANDO DE MARTINO. *La nascita delle intendenze: Problemi dell'amministrazione periferica nel regno di Napoli, 1806–1815.* (Storia e Diritto, number 15.) Naples: Jovene. 1984. Pp. xxxii, 423. L. 29,000.

These two volumes in the Jovene series on history and law focus on the Italian south, as do most of the other titles in the collection to date. Published in 1984, both studies concentrate on the administration of the Kingdom of Naples and its successor, the Kingdom of the Two Sicilies, in the years from 1806 to 1820, with the first book tracing the vicissitudes of the system of administrative justice and the second the emergence of the intendancies. Together they highlight the importance of the Napoleonic period for the Mezzogiorno, as well as for the future Italian kingdom, while exposing the obstacles to progress and the roots of the southern problem.

The first half of Raffaele Feola's volume deals with the development of the jurisdictional order of the Kingdom of Naples, which was completely autonomous and separate from the ordinary magistracy and followed the French model. The second half traces the evolution of the administrative monarchy after the restoration of 1815. Feola indicates that the French example of separating jurisdictional from administrative functions was readily accepted in the Italian south because its centralizing "political class" sought to undermine the particularism of the Old Regime. The Council of State, he reveals, served as a lever for change for the emerging political forces of southern society, advancing their interests as well as those of the French. The Gallic presence in the peninsula, particularly after Napoleon's second expedition, thus assured the triumph of the moderate, reformist elements that desired the modernization of the state structure. The conclusions reached in this section, though well-documented and clearly presented, are hardly revelations.

In the second part of the volume, Feola makes a notable contribution dispelling many of the clichés about the restored Bourbon regime in Naples by delineating its adherence to administrative reform. The administrative state, he contends, was an ideal solution for a monarchy that wished to remain enlightened though not constitutional. The dissolution of the Council of State in 1815 was the exception rather than the rule, for the greater part of the administrative structure created during the Napoleonic period remained in place. As supplemented by the legislation of 1817, inspired by enlightened southern jurists, ordinary justice was completely separated from its administrative counterpart, rendering the *Corte dei Conti* the supreme administrative tribunal and the Court of Cassation the supreme court of justice. Feola suggests that the restored monarchy's attempt to consolidate enlightened, centrist institutions was brusquely interrupted by the revolution of 1820, which was inspired by a new provincial, proprietary class with divergent aims. The new constitutionalism of this class, the author contends, was a means to affect additional changes rather than to consolidate the earlier gains. Thus, the subsequent struggle in the south was not between reform and reaction but between two versions of reform.

In his study of the emergence of the intendancies in the Kingdom of Naples, Armando De Martino examines the conflict between Paris and Naples, the capital and the provinces, the middle-class reformers and the provincial aristocrats, among others. All of this is shown to be complicated by the struggle between the new civil administration and the French military presence required to bolster the regime. To resolve the almost daily battles between the two, the government was constrained to resort to yet another tier of administrators. De Martino catalogues in clear, though pedestrian, language the legislation that created the intendancies, noting what was borrowed from the French, what southern jurists contributed, and what differences were mandated by local circumstances. He then surveys developments in the individual intendancies, paying special attention to the capital. Since the same problems emerge with distressing regularity, one cannot help but question this particular approach, which leads to considerable repetition and confusion in chronology.

The organization of De Martino's volume leaves much to be desired, and, although he and Feola have tapped a rich and diversified archival documentation, their styles are not geared to facilitate its exploitation or fully explore its implications. Nonetheless, they have produced scholarly and important works that contribute to our knowledge of the Italian south and further dismantle the "Mito del Mezzogiorno." Together they show that the south as well as the north made an institutional contribution to the unitary kingdom that emerged from the Risorgimento and both regions helped shape the course of its history.

FRANK J. COPPA
St. John's University
Jamaica, New York

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CHARLES A. JELLISON. *Besieged: The World War II Ordeal of Malta, 1940–1942*. Hanover, N.H.: University Press of New England. 1984. Pp. xii, 288. $19.95.

The siege of the island of Malta is one of the best-remembered episodes of the Second World War, though perhaps more so in Britain than in America. In recognition of its population's fortitude, the island was collectively honored with the George Cross, Britain's highest civilian award for bravery. The fortitude was real: between 1940 and 1942, Malta was under constant threat of invasion, subjected to a savage bombardment from the air, and blockaded by German and Italian air power. Air raids and the effects of disease and malnutrition killed or made casualties of perhaps 5 percent of the population, which was equivalent to a wartime loss of 7 million Americans.

Of course, Malta's involvement in the war was involuntary. It was Britain's premier Mediterranean naval base and had been a British colony since 1815. As we are reminded early in this book, Malta was not merely a colony but also one whose loyalty was so little trusted by the British that they suspended its constitution in the 1930s and ruled by decree. The reason, ironically, was the suspicion that the Maltese population was susceptible to Italian nationalism and Mussolini's anti-British propaganda. A diluted form of representative government was only restored on the eve of war.

In this fresh, lively, and interesting account of Malta's life under siege, Charles A. Jellison seeks to penetrate behind the myth of Malta's heroic loyalty to Britain, basing his account on a wide variety of archival sources in Malta as well as on oral testimony. He quotes from bitter Maltese accounts about the inadequacy of British air raid precautions and cites cynical suspicions that the British ranked the safety of the civilian population behind military preoccupations. Jellison concludes, however, that the attachment of the Maltese to their British rulers was real and unforced, if only because the Italians had forfeited all local good will by their early bombing raids. Nevertheless, he reminds us that, on a small island under siege, the British would quickly have detected any open dissent; thus, few Maltese dared voice open disloyalty.

Malta's ordeal ended with the defeat of the German armies in North Africa and the Anglo-American invasion of Sicily. (At one point, with the nearest friendly face over a thousand miles away, the psychological as well as physical isolation was extreme.) After the war, Malta remained a British colony, though one of declining strategic importance, until independence in the 1960s. It was perhaps the only British colony whose emotional bonds with the mother country were positively strengthened by the war, with significant—though temporary—political effects. For a brief moment in the 1950s, it seemed likely that Malta would be united with Britain and exchange its colonial status for representation in the British Parliament. The politics behind this and Malta's subsequent tangled relationship with Britain would make a worthy successor to this enjoyable volume, if Jellison could be persuaded to extend his Maltese research.

J. G. DARWIN
Nuffield College
University of Oxford

RAY TARAS. *Ideology in a Socialist State: Poland, 1956–1983*. (Soviet and East European Studies.) New York: Cambridge University Press. 1984. Pp. x, 299. $39.50.

This book is devoted mainly to an examination of the transformation of Marxism-Leninism in Poland, but, particularly in the introductory and concluding chapters, it includes some observations about the developments in the USSR and other communist states in Eastern Europe. Ray Taras addresses himself to what he calls an "operative ideology," namely, "the explanation and justification of policy" (p. 43). To determine this, he adopts five major criteria: the internal functioning of the Communist party, its relationship to the working class, its attitude toward religion, its involvement in scientific and cultural policies, and, finally, society's acceptance or rejection of the values promulgated by the party.

Taras traces the changes in the official ideology of the Polish United Workers' party (PUWP) by laborious research into the major party pronouncements, the speeches by its leaders, and the writings of its theorists; he also includes occasional references to the major scholarly works (mostly sociological) on this subject that have appeared both in Poland and in the West. The author ably leads the reader through all the transformations in party ideology from Wladyslaw Gomulka's ascent to power in 1956 through the years under Edward Gierek to martial law under General Wojciech Jaruzelski in 1981–83. At each stage the ideological evolution is examined in considerable detail in accordance with the five criteria listed above.

Taras's conclusion is that, although the façade of the official ideology of the PUWP has stayed intact throughout the period, at an "operative" level it has been substantially modified. It is not that the Polish Communist leaders have not sincerely subscribed to Marxism-Leninism but that, faced with persistent opposition from Polish society, they have had to abandon at least some of its tenets. In doing so, they were not entirely opportunistic, for, as the author rightly observes, Marx's famous dictum that social

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existence determines thought applies also to the Marxists.

Particularly interesting is the concluding part of the book, dealing with the role of communist ideology under martial law. The Jaruzelski regime is a strange hybrid of army generals, the main government, and party figures, all of whom, incidentally, are relatively liberal and who, moreover, enjoy the benign tolerance of Catholic church leaders. The PUWP, of course, still officially adheres to Marxism-Leninism, but at an "operative" level the major legitimization of the party's continuing in power is geopolitical rather than ideological—namely, Poland's feeble power position vis-à-vis the USSR. The author is quite correct that such a communist ideology is a "makeshift and shaky construct" (p. 258).

ADAM BROMKE
McMaster University

WINFRIED EBERHARD. *Monarchie und Widerstand: Zur ständischen Oppositionsbildung im Herrschaftssystem Ferdinands I. in Böhmen.* (Veröffentlichungen des Collegium Carolinum, number 54.) Munich: R. Oldenbourg. 1985. Pp. 536.

When Ferdinand of Habsburg acquired the Jagiello thrones of Hungary and Bohemia in 1526 because of the Turkish invasion, that same invasion determined the strategy he would have to adopt to defend them. With Hungary either occupied or a war zone, Ferdinand turned to his other lands, Austria and Bohemia, to provide the resources for defense. In Bohemia he confronted a political environment hostile to his conception of monarchical authority, a dualist system in which the estates conceived of themselves as sharing dominion with the crown, their privileges legitimated by the century-old Hussite revolt.

In this meticulous, immensely detailed study Winfried Eberhard explores the early clashes between crown and estates that Ferdinand won decisively in the 1520s. He then examines the emergence of opposition groups within the estates after 1537, culminating in the ill-fated rebellion of 1547. The conflicts that emerged in Bohemia during these years were not simply between the crown and nobility or between Catholics and Protestants but rather opposition within the elite groups defining themselves by participation in local and national assemblies (*Ständegemeinde*) against the crown and an equally large group of nobles tied to it by state service (*Beamtenadel*).

The Hussite tradition offered the estates a means to appeal to a higher law, but its effectiveness as an ideology of opposition was vitiated by internal confessional fragmentation. To begin with, the strongest faction was the conservative Utraquist group, officially recognized by the crown, generally loyal politically, and constantly on the defensive against the more radical of their number whose theological views went far beyond mere defense of the lay chalice. In 1530 the radical Bohemian Brethren, *Unitas Fratrum*, attracted a group of high nobles, who made a public display of their conversion, and formed yet a third non-Catholic party in the estates.

These divisions and the ultimately futile efforts made to overcome them provide the subject for the main body of this book. In extensive, close analyses of important political and religious tracts, Eberhard comes to the conclusion that increasing royal encroachment on traditional rights combined with Ferdinand's growing confessional intolerance to produce the coalition of reform-Utraquists, town councillors, Bohemian Brethren, and Lutherans that openly rebelled in 1547. Eberhard ascribes their failure to many factors: that they were inadequately revolutionary and confrontational, lacked number, were ideologically weak ("Bohemia lacked not only ideologies, but ideologues" [p. 497]), and lacked strategic conceptions to match Ferdinand's.

The research behind this study is impressive indeed, most of it done in Czech archives. Although much of this ground has been covered before, there is much here that is new or freshly presented with a welcome absence of partisanship. The book has many characteristics of the German *Habilitationsschrift*, the dense style of German academic social science, and the usual useless *Personenregister* in place of an index. It is nevertheless a definitive study of Bohemian history between the Hussite revolt of the early fifteenth century and the last great estates revolt of 1618–20.

JOHN P. SPIELMAN
Haverford College

GIUSEPPE RUTTO. *Bernard Bolzano: Reformkatholizismus e utopia nella Praga della restaurazione.* Turin: G. Giappichelli. 1984. Pp. 208. L. 15,000.

Bernard Bolzano was one of the intellectual leaders of the social and religious reform movement in eighteenth-century Prague. Inspired by the ideals of the Enlightenment and the reforms of Maria Theresa and Joseph II, the reformers sought to make Bohemian Catholicism more open to the Enlightenment dream of rational progress through practical religious and social action. With the coming of the Restoration, Bolzano paid a heavy price for his contribution to this movement. In 1819 his reactionary critics in church and state forced him out of his chair at the University of Prague and condemned him thereafter to a life of poverty and obscurity.

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In the scholarly world Bolzano's obscurity has long since ended, and an extensive literature is now devoted to his life and thought. No less a thinker than Edmund Husserl expressed admiration for Bolzano's work in the area of logic, and historians such as Eduard Winter have exhaustively explored the sources relevant in Bolzano's life and work. Giuseppe Rutto's brief essay, written originally under the direction of the distinguished Italian historian Ettore Passerin d'Entrèves, is a competent, scholarly exploration of the major themes of Bolzano's religious, ethical, and social thought in the context of the social and cultural developments in Bohemia from the late eighteenth century to 1848. The author has mastered both Czech and German sources and presents a balanced portrait of Bolzano as a devout but distinctly heterodox Catholic who argued that even God was bound by the "higher moral law" and was not autonomous as the Catholic theological tradition claimed.

Although Rutto's study may be a useful introduction for those who have mastered Italian but not Czech, one wonders how valuable it will be to most of the readers of the *AHR*. Bolzano is of primary interest to specialists in the history of the Austrian empire and Eastern Europe. Rutto's work adds little to the studies with which these scholars will be thoroughly familiar.

RICHARD CAMP
California State University, Northridge

JÓZSEF KARDOS. *A szentkorona-tan története (1919–1944)* [The History of the Holy Crown Doctrine, 1919–44]. Budapest: Akadémia. 1985. Pp. 246. 60 Ft.

The recent return of the Hungarian royal crown prompted a number of publications in Hungary analyzing the origins, artistic qualities, history, and significance of the country's most revered treasure. In this important and well-researched monograph, József Kardos examines how the "doctrine of the Holy Crown," a centuries-old legal concept associated with the royal crown, was applied in Hungary between 1919 and 1945.

According to tradition, the crown was given to Saint Stephen by Pope Sylvester II in A.D. 1000. It gradually became the symbol of Hungarian sovereignty. Medieval legal theory placed the crown in the center of Hungarian public law and perceived the nobles as the members and the king as head of the Holy Crown, a symbolic body. The king and the nobles together (*totum corpus sacrae regni coronae*) had the right to exercise the power of the state. After the liberation of the serfs in 1848 not only the nobles but also all Hungarians were considered to be members of the Holy Crown, which assured the survival of the concept into the twentieth century.

The purpose of Kardos's study is to show how the Horthy regime used the old concept for self-legitimation and as an "ideological weapon" for promoting internal social harmony and the revision of the Treaty of Trianon. Kardos summarizes several major theoretical works that dealt with the concept of the Holy Crown in Hungarian public law in the 1920s and describes the scholarly debates in learned journals prompted by these works. He ably contrasts the legitimists' arguments for Habsburg restoration with the ideas of those who wished to make the kingship elective or those who just wanted to continue indefinitely the existing regency under Admiral Horthy. A particular merit of Kardos's work is that he also examines several different aspects of everyday Hungarian politics connected with the doctrine of the Holy Crown. He describes parliamentary debates concerning the restoration of the upper house and the extension of the rights of the regent; he also outlines the organizational activities of the Legitimist party. The combination of theoretical issues with practical political activity makes generally interesting reading. The book's basic claim is that the doctrine of the Holy Crown was a misleading and essentially outmoded fictional historical concept in the interwar years. The author supports his thesis with an impressive scholarly apparatus and sophisticated multilevel analysis in this work.

Although Kardos is ideologically opposed to the Horthy regime, he also makes us aware of some of its positive aspects. Among others, his study demonstrates that the realm of ideas in the 1920s and 1930s in Hungary was rich and diverse and that the counterrevolutionary regime guaranteed a great deal of intellectual and even political freedom. We learn, for instance, that without losing his chair a professor of law could call into question the legitimacy of the entire political setup, which, as all Hungarian readers know, has not been possible in Hungary since 1944. It is equally interesting not only that in parliamentary debates the government could be criticized from the legitimist Right but also that the deputies on the Left could call Hungary "an operetta-like kingdom without a king" and the socialists could demand the restoration of the republic of 1918 (pp. 138, 178).

Kardos's work makes a valuable contribution to the history of Hungarian political and legal ideas, and it is also an interesting study of politics in the interwar years.

LASZLO DEME
New College
University of South Florida

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STANLEY M. MAX. *The United States, Great Britain, and the Sovietization of Hungary, 1945–1948*. (East European Monographs, number 177.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1985. Pp. vii, 195. $20.00.

Presumably, the opening of American and British diplomatic archives for the immediate postwar period tempted Stanley M. Max to reexamine Hungary's slide into the Soviet sphere of dominance. Through no fault of the author, this exercise offers only modest reward.

Max's running account of domestic developments in Hungary from the armistice to Communist dictatorship relies entirely on sources in English, principally diplomatic dipatches and State Department and Foreign Office memoranda (a notable omission is the book of "Christopher Felix," the pseudonymous American agent). This method has limitations, but it does provide occasional illuminating analysis (for example, of the fraudulent 1947 elections). Shifts in Soviet and Communist tactics are glossed over, and, despite the availability of good sources, Max misses the significance of the founding conference of the Cominform.

The work's merit lies in its depiction of U.S. and British diplomacy coming to grips with the progressive entrenchment of the Soviet-sponsored Communist party and the portrayal of the tactical differences between the two Western powers. The latter were commonly inspired by an anti-Soviet strategy, but both nations lacked the military resources (short of atomic blackmail) to compel a Soviet withdrawal from East Central Europe. The British, intent on saving Western Europe and Greece from communism, saw little merit in championing the cause of a former minor enemy occupied by the Red Army and responded with quiet diplomatic *démarches* to a succession of Soviet and communist outrages in Hungary. The American minister in Budapest, Selden Chapin, reported accurately in 1947 that British diplomacy was "hypnotized by a fantastic hope that somehow the forces of Social Democracy, which is presumed to have a sympathetic link with the British Labour Party, can be galvanized into taking the leadership in the political life in Hungary" (p. 119). Washington's more forceful protests proved equally fruitless. The Western powers could have done more to aid the nominally dominant Smallholder party—notably, with regard to Transylvania at the peace conference—but they knew they were fighting a virtually hopeless rearguard action. One can note in passing a British cabinet memorandum of March 1948 in which the writer, evidently a disciple of Sir Halford Mackinder, depicts the aim of the Soviet Politburo as "eventual control of the whole World Island" (p. 125).

I have two more quibbles with Max's presentation. First, Hungary is not generally considered a Balkan nation (p. 83). Second, that many, or most, Hungarians did not yearn for restoration of a "semifeudal capitalist system" is hardly grounds for claiming that "the 1956 uprising did not indicate a complete repudiation of the Soviet system" (p. 145). To repudiate both is, of course, perfectly feasible in theory as well as in practice.

BENNETT KOVRIG
University of Toronto

FILIP POTREBICA. *Požeška županija za revolucije, 1848–1849* [Požeška County during the Revolutions of 1848–49]. (Monografije, number 13.) Zagreb: Tiskara "Zagreb." 1984. Pp. viii, 307.

One of the more interesting ways to study a revolution is to observe its impact on a particular province or town. This book examines the revolution of 1848–49 in Požega county (*županija*), one of the six counties of the semiautonomous Kingdom of Croatia, Slavonia, and Dalmatia. Požega county came out in support of the Croatian nationalist program quite early, and it was the only Slavonian county to remain loyal to the government of Ban Josip Jelavić throughout the revolution. This richly documented book is based on extensive archival research and on published primary and secondary materials. There is also a brief historiographical survey.

Having set himself the task of explaining why Požega remained loyal to Croatia when the other Slavonian counties did not, Filip Potrebica seeks the answer in the social and economic structure of Požega county. The book, therefore, falls into two parts. The first is an exhaustive survey of the history, political institutions, tax system, class structure, and economy of Požega county and the royal free town of Požega, the county seat, on the eve of the revolution. Požega was a county of middle to large landed estates, and the land was worked by serfs. The nobles had more in common with the nobles in the Croatian counties than with the magnates of the other two Slavonian counties. Towns were craft oriented and small. Trade in agricultural goods was brisk, especially with Croatia. By the 1840s the nobles, the small middle class, officials, and the intelligentsia had joined in support of the Croatian national program, and, since the Croatian nationalists lacked a program of social reform, their linguistic nationalism and defense of traditional Croatian autonomy posed no threat to the social order.

The second and longer part of the book traces the unfolding of the revolution in Požega county from the exciting days of proclamations through the painful, turbulent problems of serf emancipation,

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the election of delegates to the National Assembly, and the establishment of the National Guard to war with Hungary. We see the everyday problems of trying to run a revolution: keeping order, finding money and supplies, keeping up morale, organizing defense. The author pays special attention to the problem of peasant unrest.

This book is rich in detail but weak in synthesis. The first part gives essential background to the second, but I am not convinced that the author accomplished the task he set for himself—to show the interrelationship of the socioeconomic order and revolutionary politics. Despite these flaws, Potrebica has written an interesting and useful book that adds a new dimension to our understanding of the revolution of 1848–49 in Slavonia and Požega county.

ELINOR MURRAY DESPALATOVIĆ
Connecticut College

DAVID MACKENZIE. *Ilija Garašanin: Balkan Bismarck.* (East European Monographs, number 181.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1985. Pp. xi, 453. $40.00.

The personal lives and careers of Ilija Garašanin and Nikola Pašić, the two dominant statesmen in modern Serbian history, have received surprisingly little attention from historians. As yet, no one has written a standard full-length biography of Pašić, but we now have a study of Garašanin covering his entire career. Garašanin served as minister of interior, foreign minister, and prime minister at different times during the 1840s, 1850s, and 1860s, the period when the administrative structure of the Serbian principality was being established and when Serbian statesmen were defining national goals. Using the extensive unpublished Garašanin private papers as well as the voluminous correspondence found in the Serbian state archives, the Ministry of Foreign Affairs, the archives of the Historical Institute, the Serbian Academy, the Czartoryski archives in Krakow, the Public Record Office in London, and published materials, David MacKenzie has produced a carefully researched, objective, yet sympathethic, study of a man whom he dubs the "Balkan Bismarck." In comparing Garašanin with the German chancellor, MacKenzie notes their similarities: both came from leading landholding families; patriotic, conservative, and well aware of their nation's limitations, both stressed national self-reliance; both saw Austria as the principal adversary of national unification. In MacKenzie's view they differed in that Garašanin was a man of principle—honest, modest, and lacking vanity or conceit—whereas Bismarck was "inordinately vain, powerseeking and arrogant" (p. 3). Yet MacKenzie concedes that, whereas Bismarck united the German lands, Garašanin was "more a man of great ideas than of practical successes" (p. 405). The unification of all the Serbian lands came only in 1918, almost a half-century after Garašanin died.

Holding high positions, Garašanin influenced almost every phase of Serbian political life. His reforms affected the economy, agriculture, communications, police, army, and medical services. In the 1840s he found himself forced to defend his weak "pitiful prince," Alexander Karadjordjević, from the intrigues of disgruntled politicians and the ever-present threat of the possible return of Miloš Obrenović, the head of the rival dynasty. Garašanin was keenly aware that Serbia's ability to determine its own future was directly related to internal stability, and he therefore attempted to assure domestic political tranquility.

In the field of foreign affairs, however, his policies were controversial. In 1844 he submitted to Prince Alexander a secret plan, the *Načertanije*, which called for the unification of Serbian lands, which he identified as Bosnia-Hercegovina, Old Serbia, southern Dalmatia, northern Albania, and the Vojvodina; Croatia and the Croatian lands, as well as the Slovenian lands, were excluded from his program. Yet in the last half-century attempts have been made, in particular by some Serbian historians—for example, Dragoslav Stranjaković and Vojislav Vučković—to present Garašanin's *Načertanije* as the blueprint for South Slav unification. They argue that not the plan alone but the policies Garašanin pursued subsequently should be taken into consideration when assessing his goals. The evidence, however, reveals that during the revolution of 1848–49, which involved the Serbs of the Vojvodina, Garašanin's principal interest was the welfare of "Serbdom." In the 1860s, when Prince Michael sought to organize a Balkan alliance and during the events that led to Garašanin's dismissal from office in 1867, he worked for closer relations with the Croats and favored some form of union with them but not at the expense of Serbia's primacy among the South Slavs or the interests of Serbdom in general. In MacKenzie's view, Garašanin was "an ardent Greater Serbian patriot" who "should not be considered a genuine advocate of Yugoslavism" but who "built sound if incomplete foundations upon which a free and dynamic Serbia and a greater Yugoslav union could later be erected" (pp. 409, 410). Furthermore, although Garašanin saw Austria as the primary obstacle to Serbian unification, he had reservations about Russian support, believing that St. Petersburg would pursue its own national interests even if that entailed sacrificing Orthodox Serbia to Catholic Austria.

CHARLES JELAVICH
Indiana University

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V. A. KUCHKIN. *Formirovanie gosudarstvennoi territorii Severo-Vostochnoi Rusi v X–XIV vv.* [The Formation of the Governmental Territory of Northeastern Rus' from the Tenth to the Fourteenth Century]. Moscow: Nauka. 1984. Pp. 347. 3 r. 50 k.

Study of the early development of Russia attracts few specialists, even in the USSR. The task is enormous, requiring the assembly of myriad small pieces of information into larger units and a detailed knowledge of sources in many languages, but particularly of the Russian chronicles. V. A. Kuchkin's work, on the territorial development of Russia during the five hundred years from the tenth to the fourteenth century, provides a framework essential for studies of that time and an introduction to later periods. He first describes the inclusion of the Volga-Oka river quadrilateral in the Old Russian state, which was followed by the consolidation of territories in that region under the forceful and shrewd Iurii Dolgorukii and Andrei Bogoliubskii, while Kievan Rus' declined until even the last vestiges of authority were moved to the northeast. By the beginning of the thirteenth century, however, the Volga-Oka region itself suffered the same retrograde trend, dissolving into virtually independent appanages. This atomization significantly aided the Mongol conquest. Under the Tatar yoke centralization halted and the region split into a number of weak principalities until the insignificant state of Moscow gradually took over the role of consolidator. By the end of the fourteenth century a significant part of northeastern Rus' had fallen into the hands of one princely line, which was a long step toward the formation of a single Russian state that, under the concept of "gathering the Russian lands," could unify the country and free it from foreign rule.

The many developments in this process are presented clearly, and the book will be essential for further study of the period. The maps, the name and geographic/ethnographic indexes, abundant documentation, and a valuable introductory chapter reviewing research on the subject enhance this valuable work.

RICHARD A. PIERCE
Queen's University

JOHN M. LETICHE and BASIL DMYTRYSHYN. *Russian Statecraft: The* Politika *of Iurii Krizhanich; An Analysis and Translation of Iurii Krizhanich's* Politika. New York: Basil Blackwell. 1985. Pp. lxxxiii, 283. $29.95.

In principle, an English edition of the *Politika* is bound to be welcome to students of Russian history, for this engaging if eccentric text is a major primary source on Muscovy in the seventeenth century. Since its discovery in the mid-nineteenth century, it has attracted the attention of such leading historians as S. M. Solov'ev, N. I. Kostomarov, V. O. Kliuchevskii, P. N. Miliukov, and S. F. Platonov. (Why the editors render the author's name in the Russian rather than his native Croatian form [Križanić] is nowhere explained, but for the sake of consistency this questionable practice will be followed hereafter.)

The edition under review is an abridged translation of the Soviet edition—itself an abridgement of the original—that appeared in 1966. The editors set out to "achieve a satisfying edited version—in the spirit of the original—rather than a strictly literal translation" (p. vi). If this is a legitimate aim, especially given Krizhanich's penchant for repetitiousness, it would have been well to indicate omissions by ellipses and also to alert the reader to places where material has been rearranged.

The text is preceded by a crisply written, eighty-page essay that takes up in successive chapters the historical background; the man and his life; Russia's political, economic, and social institutions; and the "classic preoccupations" of the *Politika*. Krizhanich's life is well presented in a chapter that relies heavily, though not exclusively, on S. A. Belokurov's masterly biography. The first and third chapters will be helpful to most readers, but the knowledgeable will find the editors' grasp of seventeenth-century Russian history and institutions somewhat shaky. We are told, for example, that prior to Krizhanich's time Russia had fought "a series of comparatively successful wars . . . for access to the Baltic (p. xlviii), that salt was necessary especially to preserve meat (rather than fish) (p. lv), that the clergy belonged to that part of the social structure called the *sluzhilye liudi* (p. xliv), and that the boyars suffered military obsolescence in the seventeenth century while the middle-level servitors readily adapted to military innovations (p. lxii).

The third chapter especially emphasizes Krizhanich's interesting though quixotic economic ideas, but the treatment is insufficiently critical, and the editors even make a dubious case for the "lasting relevance" of these ideas. The claim that Krizhanich directly or indirectly influenced Tsars Aleksei and Peter and such statesmen as V. V. Golitsyn and A. L. Ordyn-Nashchokin is also less than convincing.

The text might have been annotated more fully and more carefully. It is disconcerting, for example, to encounter such misspelled names as Reingold Heidenstein, Dmitri Herasimov, and two different spellings of Jacob Ulfeldt's name and book (pp. 116, 121).

The translation reads adequately, if a bit awkwardly here and there. A close examination of a few pages (pp. 117–19), however, turns up a disturbing number of incorrect renderings: *dvor* as "mansion"

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rather than "household"; *userdnyi* as "cordial" rather than "zealous"; *predpochitali* as "considered" rather than "preferred"; *skoree* as "always" rather than "sooner"; *praviteli* as "entrepreneurs" rather than "rulers"; *sobstvennye* as "special" rather than "[their] own." Perhaps the text overall is less error-ridden, but it is most regrettable that the editors did not have their translation checked before publishing it.

In sum, although the text conveys the gist of Krizhanich's ideas, the book is unfortunately less than satisfactory as a scholarly edition.

SAMUEL H. BARON
University of North Carolina

HANS BAGGER. *Reformy Petra Velikogo: Obzor issledovanii* [The Reforms of Peter the Great: A Survey of Research]. Translated from Danish. Foreword by V. I. BUGANOV. Moscow: Progress. 1985. Pp. 197. 80 k.

Hans Bagger's historiographical survey of the period of Peter I in Russia first appeared in Copenhagen in 1979. This Soviet edition is noteworthy in at least two major respects, the first being that it was published at all—in a pressrun of 75,000! One scans the memory in vain for another Western work on the Petrine period—on any period or problem in Russian history—to be so treated in the Soviet Union, and the publication of this work, which is properly critical of various Soviet efforts in the field and not at all Marxist in its own approach, can be seen as another sign of a new openness in Soviet historical scholarship. Equally welcome is the respectful courtesy shown to Bagger and his work by the Soviet editor, V. I. Buganov, and by the translator, V. E. Vozgrin, both of whom are also leading specialists.

Second, the Soviet contribution to this book—the forty-eight often lengthy endnotes to Bagger's translated text by A. N. Medushevskii as well as Buganov's introduction—is itself remarkable in several ways. The vigorous assertion, ostensibly on Marxist grounds, of Russia's essentially European character, an assertion that goes so far as to deny any historical dichotomy between Russia and the West, reflects to a certain degree injured national pride, regret that on the intellectual plane such a dichotomy should in fact exist, and an effort (I also think) to bridge the gap. Bagger's criticisms *passim* of Soviet dogmatism—of history that is "not the result of research proper" but of "purely theoretical explanations" (p. 154)—are not so much refuted as carefully taken note of (p. 22) and thus emphasized. Recent Western works, which either were missed by Bagger of have appeared since 1979, are cited in full—for example, my journal articles suggesting precisely the non-European nature of Russian history and questioning the authorship of the major political treatise in Russian of the Petrine period (thus undermining the work of a sizeable Soviet academic industry; to my knowledge, Medushevskii's reference to this 1981 article [p. 191] is the first to appear in a Soviet publication). First among the "insufficiently studied" aspects of the period mentioned by Buganov are the "conflict between the pre-Petrine and the new governmental systems" and the operations of the chief organ of political repression of Peter's last years (p. 23)!

About 75 entries have been added to Bagger's original bibliography of some 400 items (in both cases in Russian and Western languages), making it a most useful, if still far from exhaustive, list of scholarly works on the Petrine period. Buganov rightly notes that, while this survey is incomplete (it is particularly weak on cultural developments), it does provide a "stimulating" outline (the best available) of the "problems" of this crucial era in his country's history (p. 9). Students who read Russian rather more easily than Danish will be doubly grateful for this edition.

JAMES CRACRAFT
University of Illinois, Chicago

DAVID SAUNDERS. *The Ukrainian Impact on Russian Culture, 1750–1850*. (Canadian Library in Ukrainian Studies.) Edmonton: Canadian Institute of Ukrainian Studies, University of Alberta; distributed by University of Toronto Press, Downsview, Ontario. 1985. Pp. x, 415. Cloth $19.95, paper $14.95.

David Saunders claims that between the mid-eighteenth and mid-nineteenth centuries an intellectual convergence between the Ukraine and Russia occurred. Educated Ukrainians went to Russia, served the empire, and played a significant role in the debates about Russian national identity. They introduced the Russians to a wider Slavic world and, to some extent, prepared the ground for Slavophilism. According to Saunders, however, when the government saddled this Slavic identity with "the full weight of autocracy" (p. 253), the Ukrainian allegiance to the center began to wane and the Ukraine and Russia began to diverge.

In developing this thesis, Saunders focuses primarily on Ukrainians living and working in Russia. In fact, a more accurate title for the work would have been *Ukrainian Political and Patronage Networks in Russia, 1780s–1820s*. With the abolition of the Ukrainian hetmanate's autonomy in the 1780s, a large number of Ukrainians went to St. Petersburg in pursuit of imperial careers. The description of a newly formed Ukrainian patronage network, based in the capital and connected with the court, is the

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book's most original and thoroughly researched contribution.

That Ukrainians played a part in Russia's search for a national identity is hardly surprising, since, after the abolition of autonomy, Ukrainians sought an appropriate role within the imperial structure. Saunders does show how during the Napoleonic period Ukrainians emerged in the imperial limelight. In the sixteenth and seventeenth centuries Ukrainians were able to fuse Western elements with traditional Church Slavonic culture. By the early nineteenth century this heritage appeared to the Russians as more "authentic" or "native" than the highly Westernized culture of the court elite. It fitted well with the emerging concept of *narodnost'* and the anti-Western attitudes of the immediate post-Napoleonic era. But there's the rub. The Ukrainian contribution to the debate over a national identity was made within the context of larger questions. In addition to *narodnost'*, Saunders should have considered such other factors as the search for native roots, discovery of the folk, the development of a literary language, romanticism, idealist philosophy, and the Slavophile-Westernizer controversy. Although he touches on some of these questions, Saunders does not develop them adequately, nor does he integrate sufficiently the Ukrainian contribution with such general imperial intellectual currents. In the end the importance and magnitude of the Ukrainian impact on Russia posited by Saunders are not demonstrated and remain a hypothesis.

The work contains some additional problems. It is questionable whether all the personalities treated in the book can be included in a single Ukrainian stream of influence. For example, Saunders's argument that Ukrainians laid the ground for Slavophilism rests primarily on the writings of Z. D. Chodakowski (pseudonym of Adam Czarnocki), a Pole from the Kholm (Chelm) area, and Iurii Venelyn and Ivan Orlai, two "Ukrainians" who had originated from sub-Carpathia, which was within the Austrian empire. Do the views of these individuals have much in common with those expressed by Ukrainians from the hetmanate, or do they represent very different traditions (Polish or Austrian)?

One also cannot fully accept Saunders's conclusion that the "full weight of autocracy" caused the parting of the ways between Russia and the Ukraine. Obviously, with the arrest of the Cyril and Methodians (1847) and the subsequent prohibitions of publishing in Ukrainian (1863, 1876), the autocracy firmly blocked the Ukrainian movement. But I would argue that this was the result, rather than the cause, of the divergence between Russia and the Ukraine. Ukrainian culture, after all, was able to flourish under the banner of *narodnost'* during the rigid autocracy of Nicholas I. The parting of the ways occurred in the 1830s and 1840s when some Ukrainians reasserted views held prior to the eighteenth century and came to the conclusion that they did not simply represent a colorful variant of an all-Russian stream but were a separate people with their own language and culture worth developing and promoting. Such an outcome to the question of Russia's identity was acceptable neither to the autocracy nor to its Westernizing and Slavophile critics.

Despite such flaws, Saunders's book is the first attempt at tackling an important subject. As such, it provides us with a new perspective on the role of Ukrainians in the formation of the Russian empire.

ZENON E. KOHUT
Library of Congress

V. A. ALEKSANDROV. *Obychnoe pravo krepostnoi derevni Rossii, XVIII–nachalo XIX v.* [The Common Law of the Russian Serf Countryside from the Eighteenth to the Beginning of the Nineteenth Century]. Moscow: Nauka. 1984. Pp. 252. 2 r.

In this study V. A. Aleksandrov seeks to analyze the effects of the enserfment of the Russian peasantry on customary property law. He discusses essentially two issues: the impact of serfdom on village control of traditional landholdings and its impact on peasant customary practices of property division attendant to household partition. Aleksandrov makes no reference to examinations of customary law elsewhere, preferring to use prerevolutionary Russian scholarship in place of a comparative perspective. This gives rise to lengthy historiographic discussions of long-outdated materials. These are considerable drawbacks.

With the rise of serfdom, it was common for neighboring, but previously separate, villages of free peasants to come under the authority of a single landlord and administratively be merged into a single peasant commune. Aleksandrov is of the view that serf owners in the late seventeenth century, especially on estates where the peasants performed corvée, began to realign traditional village boundaries in order to equalize landholdings and labor obligations among their numerous adjacent villages and thus increase production. As the practice of redistributing arable lands spread, communal authority was enhanced while individual village customary control over its historic lands was reduced. Similarly, serf owners favored the equalization of obligations within a village, and they were opposed to peasant customs that inhibited this. Frequent land repartitions maintained a balance between a household's land allotment and its labor capabilities. These periodic redistributions facilitated the landlords' exploitation of the serfs, while attempting to provide for the subsistence of each individual peasant household. Consequently, Aleksandrov argues, as have many before him, that the repartitional land

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commune was of rather recent origin and related to the enserfment of the Russian peasantry. The evidence he presents gives added weight to this view.

Aleksandrov then proceeds to analyze property relations within the serf household. In one chapter, "Typology of the Peasant Family," Aleksandrov contends that, although mean household size increased somewhat between the seventeenth and nineteenth centuries, the predominant household structure was the simple, two-generation family of parents and children. Larger "undivided families," as he calls them, of three generations or with more than one conjugal pair were both fewer in number and short-lived.

Much space is given to discussing household structures in such vast and lightly populated peripheral areas as Siberia, the Far North, and the Volga region. In contrast, Aleksandrov presents but three sentences on the far more densely settled central and Little Russian provinces in which the overwhelming majority of the population lived. Not to be deterred from his belief in the predominance of the small, simple peasant household, he offers some of the strangest evidence to support his theory. Aleksandrov cites only one work, presenting in table 5 a reconstruction of data culled from I. D. Koval'chenko's well-known study of serfdom written in 1967. But Koval'chenko's original data and Aleksandrov's own version measure economic differentiation by household size. No matter which way you hold the table, it says little about peasant family household structures. Why Aleksandrov ignored the vast evidence, all to the contrary, from nineteenth-century Russian ethnographers, statisticians, officials, and serf owners remains a bafflement. Moreover, all the recent research, albeit limited, by Western scholars, using the same sources as Koval'chenko, sharply contradicts Aleksandrov's quite unfounded assertion.

Only in the final chapter does Aleksandrov turn to what is normally thought of as customary law. His sources are limited, and his discussion largely descriptive. The main conclusion is that each male had the right to establish his own small, two-generation family household. Although Aleksandrov is probably incorrect on this point, he does show that customary law guaranteed males a share of common household property. In fact, although personal property did exist, it was a weakly developed concept. Customary law limited individual authority over property. Each new household had to be given enough to survive independently. Even those not capable of working had to have their subsistence assured.

In sum, parts of Aleksandrov's study are of interest, others grossly flawed, and none startingly new.

STEVEN L. HOCH
Drew University

IU. M. TARASOV. *Russkaia krest'ianskaia kolonizatsiia Iuzhnogo Urala, vtoraia polovina XVIII–pervaia polovina XIX v.* [Russian Peasant Colonization of the Southern Urals from the Second Half of the Eighteenth Century through the First Half of the Nineteenth Century]. Moscow: Nauka. 1984. Pp. 174. 2 r. 10 k.

One might correctly suppose that a monograph devoted to Russian colonization of the southern Urals (essentially Orenburg *guberniia*) would be narrow and dry. This is no fault of the author; it is the nature of his subject. Iu. M. Tarasov's study, however, will join those numerous, valuable local histories that some day will provide the foundation for a larger, more narrative survey of the intrinsically interesting story of Russian eastward colonization.

Monographs like Tarasov's can be executed only by Soviet historians, who have intimate access to archives. Tarasov's work is the product of extensive research in the State Central Historial Archives and the State Archive of Orenburg Oblast. It is not a disputatious book, because the author is the first to examine closely the colonization of this region. The genuflections and terminological trappings common to Soviet scholarship are amply present here. But they merely appear as a rather garish façade of an otherwise substantial scholarly structure.

Tarasov's story concerns the settlement of about one million Russians in the steppe of the southern Urals during the period c. 1750–1850. The region already had a population of nomadic tribespeople, mostly Bashkir, who, in the process of colonization, were not driven off but eventually came to emulate the colonists and settle down to an agricultural life. And one of Tarasov's points is that the settlement of Russians in this region had a beneficial effect on the local people. The Russian influence raised the nomads' level of material culture. The colonists had come voluntarily, and many had indeed fled to the region from their serf masters of central Russia. The government's attitude toward this colonization was ambivalent. At one time it sought to foster regional settlement, but in the end it just tolerated this free flow of persons, of whom 84 percent were state peasants moving from state land in central Russia to state land in Orenburg *guberniia*. Tarasov describes how the rich land of the region and the freer circumstances enjoyed by state peasants (as contrasted with private serfs) combined to produce a thriving, market-oriented agricultural economy, in which many of the state peasants became substantial farmers. The book could use a map.

THOMAS ESPER
Case Western Reserve University

L. G. ZAKHAROVA. *Samoderzhavie i otmena krepostnogo prava v Rossii, 1856–1861* [Autocracy and the Eman-

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cipation in Russia, 1856–61]. Moscow: Moscow University Press. 1984. Pp. 253. 1 r. 60 k.

This is the long-awaited first systematic study by a Soviet scholar of the preparation of the emancipation legislation, "the politics of peasant reform." L. G. Zakharova was a student of P. A. Zaionchkovskii, and her work bears all the earmarks of the Zaionchkovskii school: it is a narrative based on scrupulous juxtaposition of government publications, unpublished state papers, and the diaries, memoirs, and correspondence of the principal participants, in which the documents are allowed to speak largely for themselves.

What does Zakharova's book tell us about the preparation of the peasant reform that we did not already know? One is tempted to say, not a great deal. The general contours of the story are familiar enough from a series of studies dating back to 1882. (The story is even available in considerable detail in English in my work and in Daniel Field's *End of Serfdom*, which devotes more pages to the elaboration of the government reform plans than does Zakharova's work, using essentially the same documentation).

The book is nevertheless a major contribution to the history of the peasant reform. In the first place, it traces the genesis and development of the reform legislation with a clarity and authority that easily surpass that of any of its predecessors. Although there are no great discoveries of sources, there are a great many small adjustments; the filiation of ideas, precedents, and programs is shown as never before. In the second place, the work is conceptually innovative: by approaching the legislative activity of 1856–61 as part of a continuum or process that began, essentially, in the 1840s and ended no earlier than 1883, when the allotment settlements of 1861 were subjected to obligatory redemption, the work sheds new light on a series of important questions concerning the relation between the emancipation statutes and other developments involving the peasantry in the nineteenth century, the significance of the preservation of the peasant commune in the reform legislation, and so forth.

To my mind, the most important single contribution of the book is its elucidation of the program and overall intentions of the "enlightened bureaucrats" who developed the legislation through the editing commissions (this part of the work was in fact adumbrated earlier in article form by the author). The author's hero is N. A. Miliutin, the "homme d'état russe par excellence." Zakharova clearly demonstrates, drawing on their own words, that Miliutin and his colleagues considered the emancipation statutes to be only the first step in a projected long-term government intervention in agrarian relations whose aim was to transform the communal peasantry into a class of independent, small landowning farmers—in short, the aim that was revived by P. A. Stolypin in 1906. Retention of communal tenure and the nonobligatory character of land redemption 1861 statutes were considered only transitional arrangements.

Of course, Miliutin and his friends were deceived in their expectations. Shortly after promulgation of the statutes they found themselves out of power, and before long the government turned to a policy of positive reinforcement of communal tenure, which was to prevail until the eve of the revolution of 1905 (obligatory redemption, however, was the only way out of the land relations set up in 1861, and it followed in due course). Zakharova gives short shrift to the objections raised by A. M. Unkovskii and a few other liberal gentry against the bureaucracy's monopoly on reform, despite agreement on many programmatic issues, and essentially endorses Miliutin's view that relaxation of that monopoly would only give an opening to selfish gentry interests. Yet the fate of Miliutin and his expectations after 1861 makes one wonder.

TERENCE EMMONS
Stanford University

WILLIAM C. FULLER, JR. *Civil-Military Conflict in Imperial Russia, 1881–1914*. Princeton: Princeton University Press. 1985. Pp. xxvi, 295. $39.50.

William C. Fuller, Jr., has written an important and exceptionally lucid study of the relationship between the military and civilian departments of the tsarist regime. He establishes, for instance, that from 1881 until the years just before World War I the army received a steadily declining share of government resources—from 30 percent in 1881 down to about 18 percent at the turn of the century. Fuller attributes the decline to the Ministry of Finance's program for industrial development and shows that, as a result of the program, the tsarist army received proportionately far less money than the German and French armies at a time when the modernization of weapons required enormous outlays. He also shows that the financial stringency imposed on the army generated intense conflicts between the Ministries of War and Finance comparable to the more familiar rivalry between the Ministries of Finance and the Interior.

Fuller has discovered other conflicts as well. As the burden of internal repression increased in the late nineteenth century, generals railed against the use of troops as a police force, which they claimed seriously disrupted essential training. The enormous demands placed on the army during the 1905 revolution so exacerbated tension between the Ministries of War and the Interior (and other civilian

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ministries) that officers became profoundly hostile to civilian officials and, at the suggestion of the War Ministry, compiled dossiers on the ineffectuality of the police. During the revolution and later, military judges resented and in many instances sought to circumvent the regime's use of military courts to deal with political and common criminals. The Ministry of War also resented, but could not prevent, the emperor's continuing intrusion into army personnel matters. In its conflict with the Ministry of Finance, the Ministry of War—especially under General Rediger—even sought to use the Duma and tacitly supported A. I. Guchkov's diatribes against the imperial uncles. These findings, many of which are unexpected, rest on a solid base of archival data from War Ministry files. Fuller has convincingly documented the declining position of the army within the tsarist political order and the lack of congruence between the interests of the army and those of the regime.

In fighting for its institutional interests, the military elite articulated what those interests were. Fuller's central thesis is that at the center of all of the army's conflicts with the civilian bureaucracies and with autocracy itself was a rising level of military professionalism. Fuller recognizes that the bulk of the officer corps in the late imperial period cannot be considered completely professional. Yet he argues that nuclei of professionalism did develop within the army—among the general staff officers especially—and that a professional ethos came to dominate the War Ministry. Fuller demonstrates that there was indeed a body of gifted military professionals grouped around military journals and societies and that these men did articulate their interests, which boiled down to increasing military professionalism in the officer corps and securing the army's autonomy from civilian meddling. In the aftermath of the Japanese war and the revolution, these professionals favored political and social change that would, they believed, benefit the army.

Although Fuller's thesis is in broad outline convincing, he pushes it too hard. One does not need to cite military professionalism, for instance, to account for the War Ministry's resentment of financial austerity. Fuller's assertion that during the 1905 revolution the military set its own professional training interests above the regime's interest in survival is surely an exaggeration. Fuller recognizes that the generals worried about widespread disaffection and rebelliousness among soldiers in 1905 and 1906 but insists that the real reason the generals sought to disengage the army from combat against civilians was so soldiers could train. Certainly that was one consideration. Yet one wishes that Fuller had reconciled the generals' repeated invocation of training requirements with the fact that little proper training was actually conducted (a situation that the military professionals criticized repeatedly and at length). How professional was the invocation of a professional requirement that was so little honored in practice?

Questioning Fuller's occasional overuse of the concept of professionalism is no more than a quibble when measured against the strengths of his study. One cannot read his account of civil-military conflict without recognizing that the professionalism of a sizable group of officers did pose a challenge to the tsarist political order. As in so many other areas, the tsarist autocracy thwarted the military's professional interests and aspirations and, in so doing, undercut its own base of support.

JOHN BUSHNELL
Northwestern University

V. I. KIZCHENKO. *Pervaia rossiiskaia revoliutsiia i kul'turnyi protsess na Ukraine* [The First Russian Revolution and Cultural Development in the Ukraine]. Kiev: Naukova Dumka. 1984. Pp. 187. 2 r. 10 k.

Given the dearth of contemporary publications on Ukrainian history in the Ukraine, one must welcome the rare books that do appear on the subject. V. I. Kizchenko's book focuses mainly on the events of 1905–07 and is based on a perusal of the rich tsarist police archives and on a study of much of the contemporary source material and a select number of secondary works. Its basic premise is that the Russian revolution of 1905 and the work of the Bolsheviks provided the determining impact on the Ukraine, and events that might challenge that contention are not taken into consideration. The stress placed on the leadership of the Bolshevik party, on the wisdom of Lenin, and on the impact of Maxim Gorkii illustrate the double bind of Marxist ideology and deference to the Russians. This underscores the enforced second-class status of the humanities in the Ukraine and begs the question of what indeed has changed since the tsarist regime forced the cessation of Ukrainian language publications.

Servility to Russians and downplaying of Ukrainians colors the presentation. For instance, Ivan Franko, the western Ukrainian poet who lived and worked within the more liberal confines of the Austrian monarchy (which, in contrast to Russia, did not ban the use of Ukrainian) is portrayed as opposing the Ukrainian "nationalists." The journal *Ridny Kray* (Native Land) is repeatedly mentioned without referring to the views of its publisher and editor, Olena Pchilka, who was openly critical of the chauvinism of the Russian radical intelligentsia. What for the Russians was permitted as patriotic is presented as bourgeois nationalist for the Ukrainians. That is especially evident in the first chapter of the book, which deals with the spread of education. Here the

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revolutionary elements of the Russian literacy societies are stressed, while their opposition to the use of native Ukrainian is minimized. The efforts of Ukrainians to establish a people's university are not given due credit.

Within its limitations, nevertheless, the book is useful, especially for teachers of Russian history. It makes available to the Russian-reading audience the rich tradition of Ukrainian cultural development in literature, visual arts, music, and publicism, including satire. Its presentation of the work of the Ukrainian enlightenment societies is most welcome, as is the admission that they were dedicated to the welfare of the people and not to some "nationalist" exclusiveness. The work also stresses the socially progressive character of the Ukrainian movement. It offers numerous examples of the popular basis of the Ukrainian revolutionary approach toward the workers and the peasants. Its discussion of the importance of the theater in the Ukraine, despite the severe limitations placed on it by the Russian tsarist regime, is especially useful. It even hints, as in the greetings the Kievan armory workers sent to the actress Maria Zankovetska, that the working class in the Ukraine might have been more openly Ukrainian than is generally conceded.

In short, Kizchenko provides the reader with a useful survey of the cultural events in the Ukraine that can enhance our understanding of the history of the heterogeneous Russian empire. Even with their limitations, such publications are welcome.

MARTHA BOHACHEVSKY-CHOMIAK
Johns Hopkins University

SUSAN J. LINZ, editor. *The Impact of World War II on the Soviet Union*. Totowa, N.J.: Rowman and Allanheld. 1985. Pp. ix, 300. $28.50.

This book stems partly from a two-day symposium held at Louisiana State University in April 1983, at which prominent scholars from the United States and abroad discussed "the ways in which World War II affected Soviet life" (p. 1). The editor, Susan J. Linz, provides a good introduction and an informative statistical article estimating Soviet economic losses during World War II, to which she appends a 1944 estimate by Wassily Leontief. James R. Millar has written a thoughtful summary and conclusion. The quality of the remaining eleven contributions is so high that the reviewer finds it difficult to single out the better among them.

In a methodologically sophisticated study Barbara A. Anderson and Brian D. Silver analyze linguistic and ethnic Russification among the non-Russian nationalities. Articles by William C. Fletcher on World War II's effects on religion and Deming Brown on World War II in Soviet literature are persuasive. Robert M. Slusser's discussion of Stalin's inconsistent policies toward Germany is excellent, and Jerry F. Hough's treatment of the 1945–47 debates about the postwar world must be read. Possibly the best article in the symposium is by Sheila Fitzpatrick, who in a comprehensive and penetrating manner writes about the damages inflicted on postwar Soviet *society* by the war and some of Stalin's policies.

Although the individual contributions are notable for their high and even superb quality, I am not enthusiastic about the collection as a whole. The editor has chosen a focus that is too narrow and that omits more than the impact of the war on the Soviet military, on which there is insufficient material. The shocking panorama drawn by Alexander Dallin and George Fischer in the 1950s is somehow pushed deep into the background in Linz's symposium. Only Fitzpatrick, Brown, and Fletcher do more than allude to those dark events, but even they do not bring them out sufficiently.

Hitler's attack found the Soviet Army unprepared because Stalin had convinced himself that he could manipulate Hitler. Soviet troops surrendered by the hundreds of thousands, sometimes after first shooting their political commissars. Many Soviet citizens initially welcomed the Germans as liberators. Then the miracle happened: Hitler exceeded Stalin in brutality, and the population turned against the invaders. Nevertheless, owing to a desire to escape death from starvation in the POW camps and to a continuing sense of revulsion against Stalin's policies, approximately one million Soviet nationals joined the German armed forces, among them the hero of the battle of Moscow, former Soviet general Andrei A. Vlasov. Millions of Soviet citizens worked as conscripted laborers in Germany. Stalin's secret police treated the 5.5 million Soviet repatriates—whether former German soldiers, former POWs, or former laborers—brutally, which shocked Russian patriot Aleksandr Solzhenitsyn into dissent.

In the light of these tragic conditions, the encomia addressed to the Stalinist system by Linz on page 2 ("the salient and unifying theme that emerges from this collection is the overwhelming success of the Soviet system in fighting back the German onslaught in the face of almost impervious odds initially") and by Millar on page 286 ("World War II validated both the economic and administrative structures that had been created by the Bolsheviks during the 1930s and the party itself") ring a little bit hollow. To give the devil his due: Hitler's policies helped. World War II, far from being a valid test of the Stalinist system, may have been a unique situation in that Hitler, Stalin's mad brother under the skin, destroyed his own country but drove the nearly rebellious Soviet peoples back into Stalin's arms.

YAROSLAV BILINSKY
University of Delaware

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MICHAEL BRUCHIS. *Nations—Nationalities—People: A Study of the Nationalities Policy of the Communist Party in Soviet Moldavia.* (East European Monographs, number 165.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1984. Pp. iv, 230. $28.00.

Many of the current difficulties of the Soviet regime continue to be rooted in the multinational composition of the USSR. The fundamental assumptions of Marxist-Leninist ideology about the nature and solution of the problem of nationalities have been tested and found wanting not only in the USSR but in all other Soviet-type socialist systems as well. Soviet claims about their successes in this realm have been belied by the ruthless pursuit of Russification policies, the persistence of ethnic dissension, and the growth of national self-consciousness among many of the ethnic and national groups living in the USSR.

The purpose of this small monograph is to demonstrate the realities of the nationalities policies of the Soviet regime as practiced in the Moldavian SSR. In the first part of this richly documented work, Michael Bruchis attempts to prove that one of the principal objectives of the Communist party in Soviet Moldavia has been to separate the indigenous population of the republic—spiritually, culturally, and linguistically—from the Romanian people to which it belongs and to bring about its eventual Russification. Although his account incorporates many tangential references, the author succeeds in demonstrating that the Soviet regime is engaged in a ruthless policy of Russification not only in Moldavia but also in all other parts of the Soviet Union inhabited by national minorities. This policy, motivated by Great Russian chauvinism, is often camouflaged under the ostensible objective of forging a new Soviet people. In pursuit of this objective, the non-Russian peoples are subjected to linguistic assimilation and are pressured to denigrate or abandon their distinct cultural heritage. In the case of some of the smaller national groups such as the Moldavians, the Russification policies of the Soviet regime also call for the dispersal of the indigenous population and the assignment of Great Russians to fill the party and state bureaucracies of the particular areas.

In the second part of his study, Bruchis demonstrates how Soviet historians and "scholars" serve the political objectives of the regime by distorting the origins of the indigenous Romanian population along the Prut and the Dniester rivers, by discrediting the Romanian literary heritage, and by falsifying the history of the Moldavian republic in general and that of Bessarabia in particular. He places under special scrutiny the many relevant entries in the *Enciclopedia Sovetica Moldoveneasca* (Moldavian Soviet Encyclopedia), revealing once again the wisdom of the old saying that, "behind every tyrant with a sword, there is a historian with a sponge."

The final part of Bruchis's study deals with the role Jewish Communists played in the revolutionary underground of Bessarabia and with the fate that befell them after the area's annexation by the Soviet Union. Using details that actually detract from the readability of his study, the author reveals that the membership and leadership of the local party organization contained a disproportionate number of Communists of Jewish origin—as was the case in many other parts of Europe during the Nazi-fomented anti-Semitism of the interwar period. Soviet historiography falsifies this chapter of the republic's history as well, grossly exaggerating the role of the indigenous population in the revolutionary underground movement and ignoring or minimizing that of the Jewish Communists. Although the latter served Moscow's interests in the mistaken belief that Moscow represented the cause of proletarian internationalism—a service that further fueled the fires of the anti-Semites bent on destroying "Judeo-Bolshevism"—they awoke to the Soviet reality after World War II, when Stalin reinforced the Russification policies he initiated after the Nazi invasion of the Soviet Union. But by that time it was too late, for they, like all other Jews in the USSR, had been caught in the anti-Semitic web of the Soviet regime.

RANDOLPH L. BRAHAM
City College
City University of New York

NEAR EAST

SAID AMIR ARJOMAND. *The Shadow of God and the Hidden Imam: Religion, Political Order, and Societal Change in Shi'ite Iran from the Beginning to 1890.* (Publications of the Center for Middle Eastern Studies, number 17.) Chicago: University of Chicago Press. 1984. Pp. xii, 356. $28.00.

This is an important book for two reasons: first, it brings together data and ideas that are scattered among a wide variety of books and journals; second, it challenges many of the accepted ideas about the interrelations between Ithnā 'Asharī Shi'ism and politics. Part 1 of Said Amir Arjomand's study consists of a historical review of sectarian Shi'ism from the eighth to the thirteenth century, a survey of millenarian religious and political movements in the fourteenth and fifteenth centuries, and a discussion of the theory of the ruler as the "shadow of God on earth." Part 2 deals with the establishment of an Ithnā 'Asharī Shī'ī state by the Safavids in 1501, the

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religious policies of the Safavid rulers (1501–1722), and the emergence of a Shi'i hierocracy in Iran. Part 3 discusses the impact of Shi'ism on the Qajar polity down to 1890 and ends with a general conclusion, followed by a brief epilogue on the establishment of the Islamic Republic of Iran in 1979.

The central issue on which Arjomand dissents from much Western scholarship is the legitimacy of temporal government in the absence of the hidden imam. A second matter is the question of the right of the *mujtahids* to act as the hidden imam's representatives on earth. Although the *mujtahids*' claim to be those representatives may rest on flimsy juridical grounds, it is not entirely devoid of de jure support, and the evidence pointing to the de facto recognition of this claim is conclusive. Ithnā 'Asharī political theory has always conferred on the imams the status of "those in authority" (Qur'an 4:59). Once the Shi'i community recognized the *'ulemā* as leaders of the community (940), to arrogate to themselves the position of the deputyship of the imam was an easy and natural transition for the *mujtahids*.

On the first issue, Arjomand says that "it is preposterous to suggest that all government is 'inescapably usurpatory' *in the time of occultation*" (p. 234). From the early days many pious Muslims regarded the power of government as the source of all evil and recommended that Muslims should avoid contamination by refusing to accept public office. By the twelfth century the gap between the utopian theory of a community operating under the *sharī'ah* and the reality of an Islamic empire ruled by temporal emirs and sultans (who at best paid lip service to the authority of the caliph) had become a yawning chasm, and it required the genius of an al-Ghazali to legitimize the authority of the de facto ruler. By the fourteenth century an accommodation had been reached between theory and practice in Sunni Islamic states. Many Islamicists believe that no such accommodation was made in Ithnā 'Asharī Shī'ī political theory. Arjomand claims that an accommodation was made in Qajar times and, to support his view, cites the *Tuhfat al-Mulūk of Āqā Sayyid Ja'far b. Abī Ishāq Kashfī*. But Kashfī's theory was not accepted by a majority of the *'ulemā*, and, as Arjomand admits, the theory of dual power "was not legally binding for the members of the hierocracy" (p. 229). Thus, his statement that "Kashfī's political theory can be taken to represent the unified normative order that governed the relations of authority in the Qājār body politic" remains open to question (p. 229).

Arjomand's contention that "the true sovereignty of the Hidden Imam is in no way incompatible with the legitimate rulership of a just king" is based essentially on the distinction he draws between the "millenarian extremist" or "Shi'ite-tinged Sufi extremism" of Safavid Shi'ism and "legalistically and theologically rationalized Shi'ism" (p. 234). The latter, he asserts, is quietist while the former is aberrant and unrepresentative of "true" Shi'ism. An equally strong case could be made that the Mahdistic Shi'ism of the extremists is a logical development from Ithnā 'Asharī doctrine and beliefs. I would argue that what Arjomand sees as "rejection of earthly power and pious withdrawal from the political sphere" was merely an attitude of resignation, arising from political expediency, which prevailed after Ithnā 'Asharī attempts to wrest political power from the Sunnis had failed. In other words, the whole issue of the legitimacy of any form of sovereignty other than that of the hidden imam was shelved and transferred to the messianic plane until the establishment of an Ithnā 'Asharī state by the Safavids; at that time the issue was no longer academic but became one of Realpolitik.

Arjomand's argument contains contradictions. He states not only that, "as an autonomous structure of power, the Shi'ite hierocracy was inevitably drawn into politics" (p. 263) but also that "the separation . . . of the political and religious spheres, meant that there would be no clash between Shi'ism and Persian patrimonial ethos" (p. 262). Arjomand admits that "the legitimacy of the exclusive hierocratic authority of the *'ulemā*" is still intact in the twentieth century, but he does not draw the inference that, as a result of the *mujtahids*' role as the representatives of the hidden imam, this "exclusive hierocratic authority" ineluctably leads them to claim political authority as well.

The dust jacket states that "a simplistic and misleading view of the political implications of Shi'ism was held by a number of Western scholars" at the time of the Islamic revolution in Iran and that Arjomand "refutes these distorted views." Arjomand has certainly challenged much contemporary Western scholarship and made an important contribution to the continuing debate on the subject of religion and politics in Ithnā 'Asharī Shī'ī Islam, but the quality of this debate would have been improved if he had refrained from labeling views contrary to his own as "facile" or "simplistic."

Arjomand was wise to terminate his study in 1890, for such facts as the opposition of the *'ulemā* in 1924 to Riza Shah's proposal to establish a republic and the assumption of power by the Shi'i hierocracy in 1979 would have been hard to fit into his analytical scheme. Few would quarrel with his statement that in 1979 "*ghuluww* [extremism] succeeded in conquering the Shi'ite hierocracy itself" (p. 269). But Arjomand continues stoutly to maintain in the epilogue that "nothing from within Shi'ism threatened the stability of the dual structure of domination in the reconstituted Shi'ite policy of premodern Iran" (p. 265). He is therefore driven by the force of logic to seek external causes for the triumph of extrem-

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ism, and he finds them in "imperialist penetration" and the "impact of the West on the modernization of the state" (p. 265). While not denying that they were contributory factors in the triumph of extremism, I would argue that the internal dynamics of Ithnā 'Asharī Shī'ī ideology have always contained the possibility of the triumph of this ideology in its most extreme form over secular government. By pursuing this line of argument, one may better avoid being trapped in the logical cleft in which Arjomand has placed himself.

ROGER M. SAVORY
Trinity College
University of Toronto

EMMANUEL SIVAN. *Radical Islam: Medieval Theology and Modern Politics*. New Haven: Yale University Press. 1985. Pp. xi, 218.

A fundamentalist Muslim wrote in 1951, "There is no[t] one town in the whole world where Islam is observed as enjoined by Allah, whether in politics, economics, or social matters" (p. 65). One generation later, a member of the group that assassinated Anwar as-Sadat wrote, "In all Muslim countries the enemy has the reins of power" (p. 20). Going even further, a third writer argues that Islam has not been properly practiced since the tenth century (p. 65). Thus do radical fundamentalist Muslims despair of the world around them.

Emmanuel Sivan shows what the Sunni fundamentalists in Egypt and Syria propose to do to solve this wretched state of affairs. The cure lies in political action. The same state that allows measures that take Muslims away from their religion could also bring them back to it. Radical fundamentalists call for rebellion against the state to end its pernicious influences and substitute more wholesome ones.

This course of action may not strike an outside observer as surprising, but taking up arms against the ruler goes directly against longstanding practice in Sunni Islam. In accordance with what the Prophet Muhammad himself is held to have said—that a lifetime of tyranny is preferable to a moment of anarchy—pious Sunni Muslims have historically tended to quietism in politics. To convince them to turn against their rulers, therefore, requires the development of a whole new approach to public life.

Sivan shows how the Egyptian writer Sayyid Qutb (1906–66) achieved precisely this. Some of his critical ideas came from the medieval thinker Ibn Taymiya (1268–1328), who argued that "he who forsakes the Law of Islam should be fought," even if he considers himself a proper and obedient Muslim, even if he runs the state (p. 98). Applying this rule to modern leaders who forsake the law in their chase after Western ways, Qutb concluded that they must be overthrown. The force of Qutb's thought is attested to by the attention paid to it by many radical fundamentalists in recent years, including Sadat's assassins. These justified the murder of a man watching a parade with the assertion that "the apostate must be killed even if he is in no position to fight" (p. 128).

But, as events since the death of Sadat show, killing the ruler does not alone suffice to create a properly Islamic society. To attain this, Sivan shows radicals must seize the government for themselves. Confronted by powerful and repressive states, they must go underground. Accordingly, self-contained and isolated groups of militants who live in strict accordance with Islamic precepts have sprung up in Sunni Muslim countries. These radicals constitute a source of unmitigated and potentially very dangerous opposition to the regimes they oppose.

This small but immensely learned book is the product of extensive reading in the primary sources and a deep knowledge of Islam. It makes an almost entirely unknown body of information available in English and has critical importance for the study of extremist Islamic movements.

DANIEL PIPES
U.S. Naval War College

ABDUL-RAHIM ABU-HUSAYN. *Provincial Leaderships in Syria, 1575–1650*. Beirut: American University of Beirut. 1985. Pp. vii, 220. $25.00.

In this book Abdul-Rahim Abu-Husayn provides us with an updated and detailed review of the political fortunes of the more significant local dynasties that administered Syria, Palestine, and Lebanon under the Ottoman aegis in the sixteenth and early seventeenth centuries. He describes the Sayfas, a Turkoman family that dominated Tripoli and its environs for half a century; the well-known Ma'ans, the tax farmers and landowners in Mt. Lebanon, the clan of Fakhr al-Din II, who held nearly all of Syria in revolt at one point; and the Harfush family of the Biqa', "Shi'ite village chiefs" who maneuvered between their valley fiefs. In the final chapter, three Bedouin chiefs and their families are described: the Furaykhs, the Qansuhs, and the Turabays.

The author makes good use of the traditional biographical dictionaries and consults the numerous Ottoman narrative and documentary sources, although his use of the latter is by no means exhaustive (he draws heavily on the publications of Rafek, Bakhit, and Heyd for documents). Written too much like a modern al-Muhibbi, the book still will be a handy reference for specialists needing family genealogies, but not much more. Readers looking for an analysis of the socioeconomic system that kept those families in power will look in vain. Neither

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landholding patterns nor the tribal system, nor even the ebb and flow of "desert-sown" relations that were particularly important in the latter part of this period, are given systematic treatment. Abu-Husayn makes some good points, debunking, for example, the artificial Qays-Yemeni division of Syrian social and political structures made much of by the previous generation of Western historians. But the lack of a general analytical thesis, accompanied by inconsistent, almost offhand, use of social and political terminology misleads even the genealogical story.

Consider the Qansuhs, listed by Abu-Hasayn under "Bedouin Chiefs" although later qualified by him as "probably of peasant rather than bedouin origin" (p. 182). In fact, Ottoman land records, a major archival source not used by the author, document an extensive trust deed for this family, with income drawn from town and country properties as far apart as the Biqa' Valley and the Lebanese port town of Sidon south through Bani Kilab and Ajlun of Jordan. The trust is dated 804/1401–02, well over one hundred years before the Qansuhs' emergence as a regional power under the Ottomans (Istanbul, Maliye Defter 088, p. 79). Surely this is no peasant, let alone Bedouin, family. More space for more precise use of such terms as "peasant" and "Bedouin" and less space for speculation on family and word origins would have served the reader better. If, by the way, Abu-Husayn is still interested in the origins of "Harfush," he might look at Ira Lapidus's treatment of the Harafish in *Muslim Cities in the Later Middle Ages* (1967; pp. 178–79).

Readers interested in a broader analytical picture of Syria in this period must still extrapolate from Carl Barbir's *Ottoman Rule in Damascus, 1708–1758* (1980). Perhaps in his next book Abu-Husayn will knit these genealogies into an intelligible whole.

JON E. MANDAVILLE
Portland State University

THOMAS PHILIPP. *The Syrians in Egypt, 1725–1975.* (Berliner Islamstudien, number 3.) Stuttgart: Franz Steiner. 1985. Pp. xv, 188. DM 52.

Fifteen years ago Albert Hourani published a seminal article that examined the role of the Syrian "middleman" in a changing Egyptian society. Without disturbing Hourani's basic conclusions, Thomas Philipp has now elaborated a number of the themes that Hourani set out, he has developed some new ones, and he has carried the story of Egypt's Syrian community to the present. Employing the tools of demographic history, Philipp has produced an important monograph in the growing body of migration and immigration studies.

His study examines three periods of Syrian experience in Egypt. The first begins in the eighteenth century when a major wave of Syrians migrated to Egypt. The overwhelming majority were Greek Catholics who, with the weakening of Ottoman central authority and the growth of European trade, had emerged as the middlemen between local producers and French merchants in Syria. Taking advantage of the new commercial possibilities, they moved from Aleppo and Damascus to the coastal towns, where they were employed by semi-independent provincial potentates and managed Syria's trade with Egypt. In time, Greek Catholic merchants migrated to Egypt and, owing to local developments, replaced the Jews as the premier minority involved in Egypt's Mediterranean trade. As a local minority with no official status in the Ottoman empire, Greek Catholics were attractive commercial agents for increasingly autonomous political elites. The Syrians' commercial ascendancy in Egypt did not last long, however. Muhammad Ali's consolidation of power in the early nineteenth century deprived them of their financial role in the customs administration and of their access to the new ruler.

The second period begins around the mid-nineteenth century when a new wave of migration occurred owing to the rapid growth of the cotton economy. These Syrians no longer came in great number from the bourgeois families of Aleppo and Damascus but rather were of Lebanese village origin and educated in new foreign mission schools. Nor were they exclusively Greek Catholics but included Maronites, Greek Orthodox, Protestants, and, by the last third of the century, urban Muslims who were escaping the repression of the Ottoman sultan. By the end of the century as many as thirty thousand Syrians, less than 1 percent of the total Egyptian population, may have resided in Egypt, mainly in Cairo. Although successful in the business world, they had less influence in the Egyptian economy than Greeks, Armenians, or Jews; moreover, all these Levantine communities lived in the shadow of the European business community that had established itself in the country.

Syrians had their greatest influence in journalism and government. In a chapter on the Syrian intelligentsia Philipp sensitively exposes the tensions that marked the third period of Syrian experience in Egypt. By the twentieth century, young Egyptian nationalists did not conceal their resentment toward Syrians, who were competitors for posts in the Egyptian bureaucracy. That a number of Syrian-owned newspapers supported the British colonial occupation only aggravated tensions, leading to indiscriminate Egyptian attacks against Syrians.

The interwar years brought an improvement in Egyptian-Syrian relations because the Egyptian intelligentsia assumed a paramount place in culture and government. Philipp points to new trends that

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emerged in this period. First, although Syrians assimilated quite easily into Egyptian society owing to common culture and language, their influence on Egyptian cultural and intellectual life faded. Philipp does not place enough emphasis, however, on the importance of Cairo's Syrian community to Syrian and Palestinian intellectual and political life between the wars. Second, the Syrian Christian commercial bourgeoisie in Egypt, unable to accept the secularization associated with either Egyptian or Syrian-Arab nationalism, became increasingly attached to French culture and language. This process was part of a broader effort by Syrian Christians fully to integrate their commercial activity into the more powerful European trading and financial network in Egypt.

Philipp speaks of the "de-Arabization" of Syrian immigrants as being "far-reaching" (p. 155). Their hasty departure from Egypt after Nasser nationalized their business enterprises in the early 1960s is explained more in terms of their cultural and social alienation from Egypt than in terms of their material losses. Although alienation is certainly an important factor, Philipp might have enriched his argument by examining the attraction of Lebanon. For one thing, many "Syrians" in Egypt were of Lebanese origin. Equally important, the 1960s were boom years for Lebanon, both in material and cultural terms. Its free-wheeling economy and tolerant society made it an attractive place to return to.

On balance, Philipp's meticulously researched and nicely packaged study will be of considerable interest to historians of Middle Eastern minorities, migration, and immigration. Had he compared the Syrian experience in Egypt with Syrian immigration elsewhere, this study would have been of even greater interest. Nevertheless, the author is to be congratulated for helping expose the first important link in the chain of modern internal migration in the Arab world.

PHILIP S. KHOURY
Massachusetts Institute of Technology

JEFFREY G. COLLINS. *The Egyptian Elite under Cromer, 1882–1907*. (Islamkundliche Untersuchungen, number 99.) Berlin: Klaus Schwarz. 1984. Pp. iv, 389.

Jeffrey G. Collins's book starts out unimpressively but ends rather well. An altogether too sweeping and boastful introduction is followed by two chapters that prove to be mere rehashes of sound secondary works. They prepare the reader poorly for the remainder of what is, in fact, a probing examination of the Egyptian elite during Lord Cromer's tenure of power. Although the conclusions are not new—positions of wealth and power were going disproportionately to Christians and foreigners in this period—the source material adds freshness to this study and makes it a useful contribution.

Scholars have long appreciated the vast amount of statistical and biographical material available on modern Egypt, and Collins's work reminds one that these records are still capable of much further exploitation. He uses to good effect a variety of biographical dictionaries, governmental statistical yearbooks, archival pension records about Egyptian governmental officials, and hitherto-unused Cairo property tax records to plot changes in the composition of the Egyptian elite over a quarter of a century. No doubt, statistically more sophisticated reviewers will want to raise queries about the manipulation of these data. I found his earlier chapters on demographics unconvincing, especially his reconstruction of admittedly faulty figures from the 1882 census to argue on the basis of very small percentage changes that the rural population grew more rapidly than the urban population and Lower Egypt more swiftly than Upper Egypt. Also, Collins argues that the census records suggest that the increase of the Jewish population in the late nineteenth century stemmed from immigration from Western Europe as a result of a "rising tide of anti-Semitism in Catholic Europe apparent in the Dreyfus affair at the end of the century" (p. 29). The literary sources fail to corroborate this assertion and, indeed, indicate that the majority of Jewish emigrants to Egypt were from the eastern Mediterranean. As for the increase of the European population in Egypt, a large number of these so-called Europeans were not native-born Europeans but protégés and protected local subjects of the European powers.

Far more persuasive are the remaining chapters where Collins makes use of new statistical, mainly archival, sources, along with published primary sources and secondary works. The best chapter draws on the Cairo property tax records to document a major shift of wealth and power from the Muslim population to foreigners, protégés, and non-Muslims. Subsequent chapters, based on land mortgages, royal land sales, and the pension files of government bureaucrats, demonstrate that the same shift in power was under way in agriculture and the Egyptian civil service.

Two final words of caution are in order. First, this book is not attractively presented. It is not well written and hardly seems to have undergone any process of revision from its original dissertation form. Although Collins places many of his statistics in the appendixes, too much of this material intrudes into the text and mars the narrative. Second, the causal explanations for changes in the composition of the elite are inelegant and unconvincing. Collins dismisses intellectual arguments with a sweep of the hand, flatly stating that "the topic [of intellectual life] is peripheral to the focus of this

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dissertation" (p. 1). His explanation is a political one—that the British used their power to favor Christians, protégés, and foreigners. Although that is surely the case, it is far from clear, as the author implies, that without a British occupation the Egyptian Muslim elite would have continued its steady political and economic advance at the expense of the Turko-Circassians.

ROBERT L. TIGNOR
Princeton University

AFRICA

CAROLINE NEALE. *Writing "Independent" History: African Historiography, 1960–1980.* (Contributions in Afro-American and African Studies, number 85.) Westport, Conn.: Greenwood. 1985. Pp. 208. $29.95.

The writing of African history has been suffused by strong ideological strains. At first, nationalists extolled the past in expectation of a glorious future. By the 1970s, however, the disillusioned turned to dependency theory and Marxism. Caroline Neale documents these missionary strains from 1960 to 1980, and her thesis is that nationalism was so strong that even Marxist revisionists accommodated to it. Thus, they produced schizophrenic works or dropped tenets crucial to Marxism. Since 1980, we are told, the situation has improved on that score.

A series of chapters examines nationalist historiography in relation to colonialist history, the nationalist impact on schoolbooks, its impact on academics as shown in the debate about African religious movements as expressions of political protest, and its differential impact in West and East Africa. Chapter 6 chronicles the labors of early Marxists. Chapter 5, an interlude, explores nationalism elsewhere, in India, in feminism, and in Anglo-African literature; chapter 7, the coda, claims that African oral history was quite critical of political institutions. Hence, in the African tradition it is "authentic" to be a sober, skeptical historian.

The book is obviously mislabeled. It deals with a sliver of African historiography. Neale's Africa speaks English, in former English colonies between the Zambezi and the Sahel, and is assessed from East Africa outward. Her historians are propagandists or social engineers, who exhibit those unrequited passions that so often mar historical writing anywhere. What must we think of her report that European historians want to convince, whereas African historians want to understand (p. 104)! The "independent history" of her title is ironic, for historians independent of the fashionable ideologies are doomed to oblivion. Why did she not keep the title of her dissertation—*Pride and Prejudice in African History*—adding merely "Written in English"? That would have been apt.

The author proves her argument even though chapters 5 and 7 are distracting and in chapter 4 the perceived differences between West and East African historiography are not convincingly explained and are perhaps overdrawn. Many of her statements, however, are debatable and will be questioned. From time to time she exaggerates, sometimes she reads meaning into a text where there is none, and on occasion she misses a development because it occurred in the French literature. That is true for the whole Marxist debate in French, which had started by 1960. But I found only one blatant error: The UNESCO history does not choose authors according to nationality (p. 50). Perhaps the most serious drawback is the seemingly haphazard choice of sources (rationalized on page 197). As a result, her work is a first approximation, of interest only to the informed scholar. A full treatment of the historiography of the period still awaits its interpreter.

J. VANSINA
University of Wisconsin

JOHN D. HARGREAVES. *West Africa Partitioned.* Volume 2, *The Elephants and the Grass.* Madison: University of Wisconsin Press or Macmillan, London. 1985. Pp. xv, 277. $35.00.

In 1954, John D. Hargreaves began an ambitious and, with the aid of hindsight, impossible project of chronicling the establishment of European rule in West Africa from the 1860s to the end of the "scramble for Africa." His object was to integrate imperial history with the then-nascent field of African studies, giving attention to African as well as European actors and motives. The introductory *Prelude to the Partition of West Africa* (1963) concludes with the Berlin Conference in 1884; *The Loaded Pause* (1974), the first volume of *West Africa Partitioned*, covers the rest of the 1880s, and volume 2, *The Elephants and the Grass*, takes up the last decade of the nineteenth century with particular reference to the years 1890–94. Taken together, the three studies are more than simply the most masterful, detailed, and methodical narrative of this period that we have for any quarter of the continent. Written over the past twenty-five years, the books also reflect the rapid development in monographic treatments of the African past, which Hargreaves acknowledges with his qualification and elaboration of European archival materials.

In the newest volume Hargreaves focuses on three theaters of European activity in West Africa: the Southwest (Ivory Coast, Sierra Leone, and

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Liberia), Nigeria, and Dahomey at a time he sees as the "apogee of a new form of imperialist ideology" (p. 253). It is a period characterized by European "commercial anxiety, patriotic zeal, and an overlying ethos of moral superiority" (p. 252). The year 1894 is the watershed from which the author dates the eclipse of the prior "informal imperialism," when African actors were instrumental in determining where European interests were extended, and the advent of colonies imposed by force. His treatment of the years after 1894 is accordingly less concerned with the interplay of European and African statecraft than with European power politics on African soil (ergo, Hargreaves's subtitle and punch line borrowed from Jomo Kenyatta's observation that, "when elephants fight, it is the grass that gets trampled"). One lasting impression from Hargreaves's chronicle is how transitory were so many of the diplomatic instruments: they were solemnly signed and dutifully recorded between African potentates and the newly arrived white tribes and then used or abused by either party as befitted circumstances and the needs of a moment. Wisely, Hargreaves did not feel bound by the diplomatic history that first led him into the project, and he selects data freely from fields economic and military, political and biographical, in his search for motives and for the interplay of events.

During the past twenty-five years a great deal of ink has been spilled in efforts to move beyond John Seeley's proposition that the British (or any other) empire was acquired through a fit of absence of mind. From Ronald Robinson and John Gallagher to their critics, the "scramble for Africa" has been explained by diverse events and actors, and interpretations have been sought by using diverse historical methods. Our literature on the partition era not only exceeds that for any comparable period in the continent's past but also is far more dense and, thus, in need of the valuable insights one might expect from a volume such as this. Hargreaves anticipates his critics who would seek a more comprehensive explanation of historical processes at work in the partition era than provided here by stating his own credo that "old-fashioned *histoire événementielle*, presented in the context of longer-term changes, provides the only satisfactory approach to historical explanation" (p. 249). Therein lies the integrity of the study for some readers and, predictably, a disappointment for others.

C. C. STEWART
University of Illinois

RICHARD H. DUSGATE. *The Conquest of Northern Nigeria*. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1985. Pp. 305.

The British conquest of Northern Nigeria between 1897 and 1903 was a major event in the history of West Africa, for it terminated the sovereignty of the region's largest African state, the Sokoto Caliphate and, when Northern Nigeria was amalgamated with the British territories to the south, led in 1914 to the creation of Nigeria, twentieth-century West Africa's most populous state. These events have been examined from Sokoto's side in Roland Adeleye's *Power and Diplomacy in Northern Nigeria, 1804–1906* (1971); from the perspective of the head of the chartered Royal Niger Company, which began the conquest, in John E. Flint's *Sir George Goldie and the Making of Nigeria* (1960); and in terms of their effects on the later struggle for independence in C. S. Whitaker's *The Politics of Tradition: Continuity and Change in Northern Nigeria, 1946–66* (1970).

The view adopted by Richard H. Dusgate, who became a rural development adviser in Nigeria after serving in the Rhodesian army, is more limited. The book's introductory chapters provide thumbnail sketches of African politics in Northern Nigeria and of the gradual expansion of European trade in coastal West Africa during the several centuries before the conquest. Why such broad introductions are thought necessary for readers likely to be interested in so specialized a topic becomes apparent from chapter 4 on. This book is not intended for those interested in imperialism or African history; it is simply a history of the military campaigns and is presumably intended for a British audience.

The story is ably told in clear, unsentimental prose, showing good use of the records in British and Nigerian archives. Once into his subject, Dusgate drops the strategy, used in his introduction, of presenting both African and British sides. His account is entirely from the European side, complete with inventories of the baggage trains, descriptions of the terrains of battle, and lists of the British officers and their combat decorations. Despite this narrow focus, the book is not a crude celebration of imperial victories; when mentioned, Africans are treated with respect and not subject to racial or cultural slurs, except for the author's frequent use of "pagans" to designate non-Muslim Africans.

Dusgate does not explain why he chooses to examine these battles. He attempts no assessment of what the conquest meant to the British or to West Africans. The book has no concluding chapter and only a single, endpaper sketch map. Its concern with the narrow particulars of these military events will attract few interested in imperialism or African history. Nor should it.

DAVID NORTHRUP
Boston College

TOYIN FALOLA and JULIUS IHONVBERE. *The Rise and Fall of Nigeria's Second Republic, 1979–84*. (Third

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World Books.) London: Zed; distributed by Biblio Distribution Center, Totowa, N.J. 1985. Pp. xii, 290. Cloth $30.95, paper $12.95.

The establishment of the Second Federal Republic and the elections of 1979 were greeted in Nigeria and elsewhere with a measure of hope and confidence. The country had, after all, survived some twenty years of political turmoil, civil war, and military rule; now, it seemed, Nigeria had at last put the past behind her. By adapting American constitutional and federal theory to a culturally plural society, Nigeria seemed to have designed a workable consociational democracy. Moreover, starting in the late 1960s, Nigeria became an oil exporter, and for more than a decade it benefited from the oil boom. With a double measure of dismay and disappointment, therefore, Nigeria's friends heard that on December 31, 1983, the most recent experiment in democracy had failed and that the country was ruled once more by the military.

Following that coup were revelations of government mismanagement and corruption so widespread that the regime has been called a "kleptocracy." The Second Republic may have helped alleviate ethnic tensions, but the coup showed that Nigeria's problems were endemic and that constitutional protocols were not enough to solve its many problems.

Nigeria may need structural changes and bold new policies, but, above all, the country needs rigorous and honest analysis of what ails it, and to this last point Toyin Falola and Julius Ihonvbere, both Nigerians, address their work. Their perspective is that of Marxist political economy, and they have been variously influenced by the thought of Gramsci, by the "dependency" and the "world system" schools. The book is divided into ten chapters, among which are interesting treatments of political parties and elections in the Second Republic, the economy, agriculture and the food crisis, the labor movement, external relations, and the 1983 election and the subsequent coup. The authors conclude their book with a suggestive discussion of the postcolonial state and its economic and political vulnerability in the contemporary world.

Although much of what the authors say is insightful, their analysis is not, on the whole, convincing, and their style is off-putting. Space does not allow a detailed critique of their work or their approach. Suffice it to say that in this case, as in so many others where Marxist or other grand theories are applied to local conditions, there is a big risk of reification, of seeing as real theoretical entities that may not exist in real life. Thus, the authors spend much of their time blaming "the political class" and its hangers-on, the Western-educated university intellectuals, for many of Nigeria's ills. This "political class," which is apparently composed of everyone including the military, who has ever held power, is said to be corrupt and to act as an agent of foreign capitalism. Besides not being novel, the problem with this analysis is that it assumes the existence of something that needs to be established. Is there really such a thing as a political class in Nigeria? Do members of that class act in concert to defend their interests? Since the Nigerian political collectivity is badly divided along sectional, ethnic, and patrimonial lines, how can one still speak of it as a class? Indeed, if blame is to be apportioned for Nigeria's ills, how can one scold such an amorphous entity? This work is helpful in identifying some symptoms of Nigeria's pathology, but, as a diagnosis of the country's ills and as a prescription for the future, it is not convincing.

ROBERT MELSON
Purdue University

DEREK NURSE and THOMAS SPEAR. *The Swahili: Reconstructing the History and Language of an African Society, 800–1500.* (Ethnohistory.) Philadelphia: University of Pennsylvania Press. 1985. Pp. vii, 133. Cloth $20.00, paper $11.95.

Since the nineteenth century the belief has persisted that the Swahili civilization of the East African coast was molded by Arab and Persian influences. Derek Nurse and Thomas Spear have joined forces in this volume to argue that the Swahili are an African people and their language is basically Bantu. If the Swahili have borrowed freely from others, they argue, the influence has gone both ways, resulting in a synthesis of African and alien ideas within an African context. In taking this stance, the authors are not alone among recent scholars of East African history. By gathering recent interdisciplinary research and reassessing primary sources and earlier studies, however, their book serves to clarify this point of view.

The authors divide their study into two periods, the first from A.D. 800 to 1100, when agricultural Swahili became distinguishable from their neighbors and migrated down the coast to found villages and towns. Historical linguistics is a key means of reconstructing a model of the protohistoric Swahili, and nearly half of the book is devoted to this approach. Recent excavations of settlements in the Lamu archipelago, dating from the ninth century, support the linguistic findings. Evidence of Arab influence before 1100 is slight.

The second period extends from 1100 to 1500, when the Swahili town reached the peak of its development as an indigenous form. The authors conclude that what set the Swahili apart from their neighbors was not alien origins or culture but the

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"phenomenon of a maritime trading diaspora and the urbanization, economic differentiation, and social stratification that resulted from trade and migration" (p. 97).

This brief and readable book may be useful as an introduction to the study of Swahili society, in part because it comprises a good review of Swahili historiography. The bibliography, though not exhaustive, is extensive; maps and tables are clear. The seven linguistical appendixes, however, may be of more interest to the specialist than to the beginner, as will the sometimes speculative interpretations of the sources on a number of topics. Among these are the extent of Swahili cohesiveness and the significance of Shungwaya in southern Somalia as the Swahili (including Shirazi) homeland; one might note that the authors have minimized regional diversity in creating a generalized model of Swahili society. Some intriguing issues, such as the role of Islam during the centuries before 1500, remain elusive, but Nurse and Spear have clarified much.

MARGUERITE YLVISAKER
Albert Lea, Minnesota

RICHARD PANKHURST. *History of Ethiopian Towns from the Mid-Nineteenth Century to 1935*. (Äthiopistische Forschungen, number 17.) Stuttgart: Franz Steiner. 1985. Pp. 391. DM 126.

Towns have been an important feature of indigenous Ethiopian civilization since at least the seventeenth century, but scholarship has failed to do justice to this fact. A dominant tradition within social science actually denies the existence of urbanization in premodern Ethiopia and emphasizes the transient character of such concentrations of population as did from time to time occur. Historians have failed effectively to challenge this tradition. Richard Pankhurst apparently sets out to do so in this book and in its companion volume, *History of Ethiopian Towns from the Middle Ages to the Early Nineteenth Century* (1982), but one cannot be quite sure since he makes no explicit reference to the arguments of the skeptics. Consequently, Pankhurst merely contributes toward an argument that others will have to make.

At its best Pankhurst's book is a remarkable assembly of information thematically organized with special attention to questions of population, physical character, and economic activity. Smaller centers receive considerable space because the book's basic organization proceeds from town to town. Pankhurst seems to espouse the view that Ethiopian towns originated as administrative centers and, prior to the twentieth century, owed their ongoing existence largely, or wholly, to the presence of feudal rulers. As a result, he directs a good deal of attention to political events occurring in and around his selected towns. He conducts his enterprise using a characteristically broad set of sources, but he bases his account overwhelmingly on the published sources and makes only occasional reference to archival collections and oral informants.

Unfortunately, the book's usefulness is seriously limited. Pankhurst eschews theoretical and comparative issues and places his material before us with few clues as to its significance. He exercises insufficient critical judgment in reading his sources and in selecting his material. Stylistically he relies far too much on lengthy extracts from the sources, extracts that are often repetitive. He does too little to shape his material. He apparently believes it his duty to report every population estimate contained in his sources but not to pass judgment on those estimates. One of his few constructs, derived from a source, is the working assumption that each dwelling of a premodern Ethiopian town contained 6.5 persons (p. 104). This figure, which is dubious and inflated, is, moreover, contradicted within the book. He also commits basic errors in the historian's craft. The first pages of text (pp. 41–42) contain a gratuitous error in the use of sources: Pankhurst has the newly crowned Emperor Theodore march to the imperial capital, Gondar, following his coronation in February 1855. This "incident" rests on retrospective accounts, and the earliest sources tacitly deny the visit. That Pankhurst reports the incident is, in any case, gratuitous: it tells us nothing of the town as a town nor anything of its changing fate. Indeed, Pankhurst includes far too much unnecessary material in this book, which is a product of the "scissors and paste" school.

DONALD CRUMMEY
University of Illinois

ALLAN H. JEEVES. *Migrant Labour in South Africa's Mining Economy: The Struggle for the Gold Mines' Labour Supply, 1890–1920*. Montreal: McGill-Queen's University Press. 1985. Pp. xiv, 323. $30.00.

The growth and prosperity of South Africa, that continent's only significant economy, has always depended on the labor of blacks. The exploitation of long underground ribbons of gold provided and still provides its main fuel; without the backbreaking work of Africans, much of the world's gold would have remained in the deep and sloping ground. Allan H. Jeeves's book is a narrow but important examination of how, during the industry's first thirty years, Africans were enticed and dragooned into doing the unpleasant and brutish tasks that whites would not perform.

Jeeves's book is not really about labor conditions, the onerous and dehumanizing compound system,

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migrancy generally, or the place of gold in the regional economy. Nor does Jeeves provide enough commentary on those individual decisions that in aggregate resulted over time in an abundant and regular flow of contract and voluntary workers to dig the gold that made white South Africa rich. But the study's great value is the systematic way in which Jeeves analyzes the mineowners' recruitment of a labor force, the anticompetitive policies that they followed (and why), and the role of government in facilitating a smooth flow of employees and limiting market forces among Africans by enacting and enforcing pass, vagrancy, and antidesertion statutes.

Jeeves's careful dissection of the vicissitudes of supply-and-demand questions concerning labor during the gold industry's founding decades adds intelligently to the vigorous, ongoing debate about the true character and motives of the capitalistic exploitation of African labor. Jeeves demonstrates that, contrary to received wisdom, the mine monopolists controlled their supplies of labor with difficulty. It took them thirty years to secure their interests in a stabilized supply of inexpensive, unskilled workers. Competition between the various companies flourished despite the efforts of the Chamber of Mines and the actions of the government. Africans persisted in exercising choice, if not solely on the basis of price, then certainly on the basis of some kind of recognizable total return. The virtues of monopsony were clear to the managers, but theory was always subordinate to profit in the immediate term; production targets had to be maintained, and a steady stream of workers, who went down into the mine to drill blasting holes by hand along the ore faces, was essential on every shift.

It is clear that Africans would not have sought employment underground if they had not been taxed or if their ability to expand an indigenous agricultural base had not been severely limited by local and then regional governments. Even so, Jeeves rightly judges the pace of proletarianization as slower, more uneven, and less capable of shunting labor to low-wage mines than has hitherto been suggested. Indeed, Jeeves courageously cites the economic benefits (cash and cattle advances) that rural districts and some groups of Africans received from labor recruitment efforts in the period immediately before Union.

Africans were not attracted to the mines by the nature of the employment or by the comparative attractiveness of their wages. Only after the passage of the Natives Land Act of 1913 and the creation of reserves did coercive measures of all kinds begin to have the effect that mineowners had sought since 1886. Until that time the Randlords steadily competed, as Jeeves shows so well, for workers and blustered more than they dominated. The Chamber of Mines' recruitment agency failed repeatedly to control the labor supply until after World War I because the mining houses differed among themselves on substantial policy issues.

By rising above (but never forgetting) the human actors and human tragedies represented by and encapsulated in the brutual saga of migratory labor, Jeeves may be accused of having written a social history that is too abstracted and too removed from the battlefields of consciousness and class. But, by exploring the origins of labor policy in detail, he has put the study of the wider impact of early South African gold mining on a firm empirical foundation.

ROBERT I. ROTBERG
Massachusetts Institute of Technology

ASIA AND THE EAST

SUSAN B. HANLEY and ARTHUR P. WOLF, editors. *Family and Population in East Asian History*. Stanford: Stanford University Press. 1985. Pp. xx, 360. $45.00.

This conference volume marks an important advance in the comparative historical demography of China and Japan. The papers on Japan (with the exception of a study of an outcaste village population by Dana Morris and Thomas Smith) portray an eighteenth- and nineteenth-century demographic regime of moderate mortality that responded quickly to economic downturns through control of marriages and births. Kozo Yamamura's privileged Tokugawa bannermen, for example, reduced the size of their families when faced with economic decline. In Susan B. Hanley's Okayama villages younger sons rarely married, and the marriage of daughters was postponed in bad times. The village population in Akira Hayami's Nishijo was held constant through out-migration. Urban households were small and ephemeral, and they had a high percentage of solitary families, as Robert Smith shows for Tennōji, or were plagued by a low general fertility rate, as Yōichirō Sasaki shows for Takayama.

The excellence of the Japanese historical data—of the six papers, five are based on the annual household registers compiled during the Tokugawa period (1600–1868)—stands in sharp contrast to the Chinese sources. Only two papers by Ts'ui-jung Liu and Stevan Harrell, based on lineage genealogies from east China, provide any historical depth. Liu concludes that the high mortality, low marital fertility, and low rate of natural increase found in materials (dated 1650–1849) are plausible when compared with the model tables. Harrell looks at the positive correlation of lineage segmentation with

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elite status and population growth from 1550 to 1850. Both authors note the flaws of the genealogies: the nonreporting of females, the unreliable coverage of males who die before adulthood (and marriage), and the omission of infertile concubines.

The household registers compiled by the Japanese, who governed Taiwan from 1895 to 1945, are comparable in quality to the Tokugawa registers. Sophie Sa uses registers from the city of Taipei to demonstrate that high status was positively correlated with the major form of marriage and negatively correlated with age at marriage. A surprising by-product of her analysis concerns the use of the less respectable minor and uxorilocal forms of marriage by all social strata. The variability of local practice is underlined by Burton Pasternak, who argues that some villages on Taiwan's central west coast practiced uxorilocal marriage as a means of recruiting extra farm labor and that these marriages were no less stable or less fertile than the major form.

The poor quality of data for China fuels the debate between Ansley Coale, who defends the general usefulness of the corrected Buck survey of forty thousand farm households (1929–31), and Arthur P. Wolf, who attacks it. Demographic historians now working on new sources such as the household registers for Liaoning and the Ch'ing imperial genealogy will have to respond to the themes raised by this volume's authors and editors. Future research should first address the question of why the Japanese replicated the West European demographic pattern rather than the more common patterns of early and universal marriage and grand rather than stem families found in China and Eastern Europe.

EVELYN S. RAWSKI
University of Pittsburgh

NG CHIN-KEONG. *Trade and Society: The Amoy Network on the China Coast, 1683–1735*. Singapore: Singapore University Press. 1983. Pp. xxiv, 318. Cloth $17.00, paper $11.00.

It now is generally recognized that studies of late imperial China must take into account the important differences in economies, societies, and patterns of development and decay among the regions of the huge empire. Other scholars, including myself, have already attempted to sketch the contributions that can be made to such a "regionalized" historiography by studying "maritime China," which had its core in maritime Fujian and outlying areas in other coastal provinces, Taiwan, and even such major centers of overseas settlement as Luzon and Java. Ng Chin-keong's intelligent and thoroughly researched book is a major contribution to this important historiographical trend. Offering information on commercialization, urbanization, and their social consequences; on eighteenth-century despotism, reform, and their limitations; and on distinctive patterns of central control and local government, this book will be of great interest not just to specialists of maritime China but also to every student of the Qing economy, society, and government and to the growing number of scholars interested in the interrelated maritime histories of Asia from Aden to Pusan.

Ng has marshaled a rich array of sources and secondary literature to develop a coherent picture of the trade network along the coast and with Taiwan, centered on Amoy (Xiamen), that expanded rapidly after the incorporation of Taiwan into the Qing empire in 1683 and the subsequent legalization of maritime trade. (Amoy's trade with foreign ports, which is known in outline from earlier studies but requires extensive work in European-language archives for a fuller understanding, remains in the background of Ng's work.) Many of the author's subjects—frontier settlement on Taiwan, grain supplies from Taiwan and from the Yangtze valley for grain-poor coastal Fujian, Amoy as a central point for coastal and overseas trade, changes in official policy toward trade with Taiwan and with foreign ports, the wary attitude of the Qing court toward a maritime world that had produced its most tenacious opponents in the conquest decades—will be familiar to scholars who have explored the field, but on every topic Ng has new sources and is admirably clear and coherent where some of his forerunners have been muddled or annalistic. His account of the distinctive social formations of Amoy as a city of migrants and merchants with an important military command but no local magistrate of its own is a fascinating contribution to the urban history of China. Ng draws on archival documents published in Taiwan to compile a very impressive series of appendixes giving full cargo information on 143 ships that traded from Amoy to Tianjin between 1717 and 1732 as well as some intelligent estimates of the values of these cargoes. This is the best quantitative information on shipping that I have seen from a Chinese source.

Much remains to be done on the topics Ng discusses and on other topics in the history of maritime China. Archival material must still be explored in Beijing, The Hague, Jakarta, Seville, and so on. Good work is being done in Taiwan and in Amoy itself. As studies of maritime China continue to grow, Ng's book will remain a landmark in the maturing of the field.

JOHN E. WILLS, JR.
University of Southern California

BENJAMIN A. ELMAN. *From Philosophy to Philology: Intellectual and Social Aspects of Change in Late Imperial*

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China. (Harvard East Asian Monographs, number 110.) Cambridge, Mass.: Council on East Asian Studies; distributed by Harvard University Press, Cambridge. 1984. Pp. xxv, 368. $20.00.

Benjamin A. Elman's book is a social and intellectual treatment of a major shift in Confucian discourse in late imperial China. Such major shifts had occurred twice before: first, during the Han, when Confucianism emerged as the state creed, and again during the Sung, when Neo-Confucianism emerged, infused with Buddhist metaphysics and Taoist metempsychosis. Elman views this third shift as a revolution in thought. No doubt, there will be others who would rather see it as an internal displacement in the Confucian universe of thought and action. Whatever the interpretation, the important point is that such major shifts reveal the nature of the Confucian state and society in which texts, forming the bases for state doctrines and social creeds, came under serious scrutiny when major social and political changes occurred.

The Ming-Ch'ing changeover was just such a time. Elman adroitly charts the veering away from Neo-Confucian moral speculation (*tsun te-hsin*) toward an appreciation of practical learning (*tao wen-hsueh*), which, in textual terms, was called evidential learning (*k'ao-cheng hsueh*). In six chapters Elman explores the intellectual and social aspects of *k'ao-cheung*, with social factors receiving greater emphasis. This is, perhaps, the signal achievement of Elman's considerable labors.

For the first time in English and in comprehensive detail the institutional and social story of Kiang-nan (Yangtze River South) scholarship is told. The major theme in this sector is the professionalization of academics and the existence of a community of scholars with occupationally defined skills. Elman systematically examines the sociology of scholarship: social origins of the scholars, official and semiofficial patronage, academies, the roles of the literati and their transformation, libraries, printing and publishing, reference materials and evidential research, and classification of knowledge. In addition, Elman illuminates the means of scholarly communication: notation books in shared research techniques; the uses of data; scholarly collaboration, correspondence, and meetings; cumulative research; priorities and notions of progress in research. All of these institutions and practices support what Elman calls a "shared epistemological perspective." They further support Elman's claim that European-style science could have arisen in China but for the inaccurate representation of it by the Jesuits and the priority given to philology (pp. 82–85).

The last chapter, the denouement as it were, tells of the shattering of the "shared epistemological perspective" by the rebellions in the mid-nineteenth century. It also touches on another theme that is gently suggested throughout the book: on the eve of its encounter with Western society, Confucian China, far from being the decaying victim of the dynamic West, had actually worked out the proposition of "truth through practice," an idea that persists even in contemporary China. Ideals survived the destruction of institutions.

Students interested in the regenerative capacities and scholarly standards of Confucianism and its intellectual legacy will find this book both rewarding and essential reading.

D. W. Y. KWOK
University of Hawaii

DIANA LARY. *Warlord Soldiers: Chinese Common Soldiers, 1911–1937*. (Contemporary China Institute Publications.) New York: Cambridge University Press. 1985. Pp. ix, 177. $29.95.

China in the twentieth century, Diana Lary writes, "found herself, if not in a general reign of terror, at least in a prolonged period of chronic violence" (p. 101). As China's political order broke down, it became a nation ruled by the gun. Warlord armies, militant secret societies, bandits, and defense corps became fixtures of the local landscape.

Although the study of warlordism has expanded greatly since James E. Sheridan published the first monograph in the United States on the subject twenty years ago, most of the research has focused on the careers of the principal warlords, the dynamics of warlord politics, and warlord ideologies. In these studies the common foot soldier is always there but in the background. To Chinese living at the time, however, the ideology of the local warlord was of little obvious consequence. They experienced warlordism above all in their encounters with the warlord soldiers.

Lary has few kind words for these armed merceneries. "Looting and theft satisfied [their] greed," she remarks; "rape satisfied [their] lust. But many of the acts of violence which occurred with sickening regularity through the warlord period were . . . manifestations of a love of violence for its own sake, casual, pointless acts of spoiling and destruction" (p. 79). Most warlord soldiers were peasants driven from farms by poverty, boredom, and grinding toil. They were also attracted into the army by the prospect of wealth. Although wages were low, the opportunities for sudden windfalls were numerous: extortion from villagers could provide them a succulent meal; good fortune in pillage could make them rich; and just selling a gun to the rival army or to a neighboring bandit gang could bring them eighty *yuan*—far more than they could accumulate in a year or two working on the soil. In

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the chaotic society of twentieth-century China, therefore, soldiery had become a major avenue of upward social mobility.

In successive chapters Lary discusses the warlords' methods of recruitment, the soldiers' life in the army, relations between the common soldiers and their officers, links between soldiers and bandits, and the mentality of the soldiers, which impelled them to such brutal and violent behavior. Interestingly, Lary suggests that one reason soldiers acted as they did was that they felt anger toward a society in which, before joining the army, they had been at the bottom of the pecking order. Thus, "any sense of common origin, of class ties between soldiers and civilians, was destroyed by the power they had as armed men over unarmed ones" (p. 89).

This is a small book—only 111 pages of text—and the reader might wish that Lary had examined the phenomenon of violence and militarization in the twentieth century more deeply and broadly. Banditry, for instance, which was probably more pervasive and destructive than warlord soldiery, still awaits thorough study. And the controversial, but profoundly important, question of how seriously warlord violence affected the economy and the peasants' standard of living is not broached here. (Lary has discussed the topic elsewhere, but not definitively.)

Perhaps, however, we ought not be too greedy. For, as it stands, *Warlord Soldiers* already contributes to our understanding of the armies that for over two decades swarmed across China robbing, raping, and vandalizing wherever they went.

LLOYD E. EASTMAN
University of Illinois

HERMAN OOMS. *Tokugawa Ideology: Early Constructs, 1570–1680*. Princeton: Princeton University Press. 1985. Pp. xvi, 349. $37.50.

Herman Ooms persuasively refutes the traditional view that the founder of the Tokugawa bakufu, Ieyasu, adopted Neo-Confucianism as the ideology to legitimate his rule. This was a myth fabricated by Hayashi Razan's descendants in the 1660s to create the image of Razan as the house scholar. Early Tokugawa thought, Ooms asserts, was not a monolithic transplant of Sung philosophy but was varied and pluralistic; Shinto, Buddhism, and folk beliefs were just as important as Confucianism in the intellectual order.

In his analysis the author makes imaginative use of the concepts of structuralism, deconstructionism, and other theories. He explains that his study originated as an examination of Yamazaki Ansai's thinking in order to understand Tokugawa ideology, but he discovered that he had to go back to early Tokugawa thinkers and then further back because many elements of Tokugawa ideology predated Ieyasu by several decades. Thus, he leads us back to Nobunaga and Hideyoshi, who sought to legitimate their reigns, which had been established by force, by claiming divine authority. These warlords used Shinto and Buddhist concepts to sacralize their authority. Ieyasu followed suit.

Ieyasu was more interested in sacral than in discursive legitimation, but scholars who aspired for a place at the center of power concentrated on the latter. Although eclectic in their thinking, Tokugawa scholars focused on the cosmos and man—specifically, on his psyche and mind—to fit him into the newly established polity.

In examining the ideological constructs of early Tokugawa, Ooms centers his attention on Fujiwara Seika, Razan, Suzuki Shosan, and Ansai. He contends that, although Seika and Razan have been credited with converting Ieyasu to Neo-Confucianism, they hardly influenced the shogun, who was more interested in Tendai Buddhism and Shinto. Tenkai, a Tendai monk, was more properly a bakufu ideologue than Razan was. He played a central role in the deification of Ieyasu as the incarnation of Amaterasu, the Sun Goddess. Suzuki Shosan, another Buddhist, was also more influential than Razan in upholding the social order; Shosan admonished the peasants that their karma was to work diligently at their prescribed task.

The centerpiece of this study is Ansai, whose writings Ooms analyzes in meticulous detail, leading us through the "lush semiotic flora" of the philosopher's literature. Ansai began as a Zen Buddhist but turned vehemently anti-Buddhist and became a fanatical believer in Chu Hsi philosophy. He was, Ooms claims, the first scholar to introduce Chu Hsi thought correctly into Japan. Ansai also emerged as a Shinto nationalist who saw a complete ontological unity between Neo-Confucianism and Shinto. By apprehending "what is under the text," he found Neo-Confucian truths in Shinto texts. In constructing his multifaceted epistemology, he, like the patristic scholars of medieval Europe, created an allegorical system with four levels of meaning: literal, allegorical, moral, and anagogical. A believer in the ontological oneness of things, he perceived heaven and man to be the same. Hence, total loyalty to the ruler was mandatory. A stern disciplinarian, he preached a moral rigorism that required one to be ever-vigilant in preserving purity of the mind and a sense of reverence. Convinced of the rightness of his concepts, Ansai was intolerant, exclusivistic, and fanatical in condemning anyone who did not fully agree with his interpretations.

Ansai's philosophy was accepted by the high bakufu officials, Ooms believes, because of the systematic structure of his thought. Ansai thus became

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the true creator of Tokugawa ideology and the forger of "ideological closure." Before Tokugawa rule, Ooms argues, there was no ideology because there was no class confrontation. The Tokugawa order, however, was a system of domination designed to exploit the peasants—hence, the formulation of an ideology.

Ooms has produced a work impressive in its erudition, depth of scholarship, and theoretical exegesis. He has examined a prodigious amount of literature and displays a superb mastery of arcane Japanese terms. The theoretical complexities, the detailed dissections of the writings, the reiterations, and the specialized terminologies make the reading heavy going for nonspecialists, but the intellectual rewards are rich indeed. Like Ansai, the author passes stern judgments on those with different perspectives, but he is never acerbic or strident. Henceforth, no one can discuss Tokugawa thought or history without taking account of this provocative and challenging work.

MIKISO HANE
Knox College

NEIL MCMULLIN. *Buddhism and the State in Sixteenth-Century Japan*. Princeton: Princeton University Press. 1984. Pp. ix, 441. $45.00.

That Japan achieved a political and religious synthesis of Buddhism, Shinto, and Confucianism, which was called the Ritsuryō system, during the sixth and seventh centuries is well known. After Prince Shōtoku's death, the Japanese nation became highly centralized, following the Sui and T'ang model; bureaucrats trained in Confucian learning administered the affairs of state; and the sovereign came to be regarded as the "manifest *kami*" by virtue of his descent from the Sun Goddess, while someone other than the emperor ruled in the position of regent, as Shōtoku had done. The result was a form of "immanental theocracy."

The Ritsuryō system had many components. The state, for example, accommodated Buddhism through its *ōbō-buppō* doctrine of mutual support or dependence. Buddhism, for its part, advanced a theory known in Japanese as *honji-suijaku*, which interpreted the Shinto *kami* as manifestations or incarnations of the original nature of bodhisattvas and the Buddha. This theory was formulated precisely to justify the pattern of coexistence between Shinto and Buddhism known as the *Ryōbu-Shintō* or *Sannō-ichijitsu-Shintō*. In other words, both Shinto and Buddhism were transmuted, thanks to newly created myths that sanctioned their amalgamation and served the cause of an immanental theocracy permeating every aspect of national life. Although it underwent many internal changes, the remarkable and complex Ritsuryō system was resilient enough to last until the Onin War (1467–77) and the Sengoku ("country at war") period that followed.

During the second half of the sixteenth century, three strong men successively rose to power and tried to unify the nation politically: Oda Nobunaga (1534–82), Toyotomi Hideyoshi (1537–98), and Tokugawa Ieyasu (1542–1616), who established the Tokugawa feudal regime in 1603. It is important to understand that regime as a second great synthesis that the three unifiers all helped to usher in but one that Neo-Confucianism rather than Buddhism explained and, in a sense, "authenticated."

Neil McMullin concentrates on the first of the unifiers, Oda Nobunaga, which has not been done before, and makes a real contribution to our understanding of the dramatic shift in Buddhism's relation to the state. His reasoning is correct, although McMullin is so interested in Oda that he sometimes gives the impression, as in part 3 of the book, entitled "The Result," that Toyotomi and Tokugawa followed their predecessor's lead implicitly. It seems worth noting that both were men from different backgrounds, religious and otherwise, and with different temperaments.

JOSEPH M. KITAGAWA
University of Chicago

RAJAT KANTA RAY. *Social Conflict and Political Unrest in Bengal, 1875–1927*. New York: Oxford University Press. 1984. Pp. 398. $32.50.

Rajat Kanta Ray has produced a fine work on Indian nationalism in Bengal and on the modern history of this region. His major achievements are to explore the economic and political history of Bengal between 1875 and 1927 and to examine the careers of British imperialists and Indian nationalists. The author was trained at Cambridge University under Anil Seal, leader of the Cambridge historians, who reduced the history of nationalist India to the strivings of Indian patrons and clients trying to replace their benign British rulers. What is lost by this approach is an overarching idea of the interplay of nationalism and imperialism. Indians are seen as motivated primarily by economic self-interest.

In the present work Ray goes beyond the limitations of the Cambridge school and masterfully shows how British bureaucrats and businessmen ruled India and how Indians driven by economic interest and political motives, selfish and selfless, fought to regain control of the economy and polity of their land. He also shows the limitations of the approach of J. H. Broomfield, whose *Elite Conflict in a Plural Society* (1968) stressed the self-seeking role of an undifferentiated elite group that he called the "bhadralok." Ray has a surer grasp of the period,

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and he differentiates between the partisans and groupings within British India and Indian society.

The author deals first with European business in Bengal, where it was especially concentrated. He shows how these business interests shaped the economy and polity of Bengal, successfully preventing Bengalis from entering business or from having a significant say in ruling their own province until after World War I. Then Ray moves to a more chronological account of the politics between 1875 and 1927, with occasional comments on economic developments. He believes that economic rivalry between Britain and India was at the root of political conflict and racial antagonisms.

Ray is strongest on linking economic and political factors and weakest on the psychology of the colonized. What he does not see is how the oppressed often adopt the ideals and values of the oppressor, striving to be like the oppressor while also fighting against him. Ray also differentiates between what he calls the "natural" and the "distorted" development of India. This rather strange dichotomy leads him to call selfish measures by British bureaucrats and businessmen "distortions" when, in fact, his own data demonstrate that these moves were shrewd if narrow-minded attempts to hold on to the empire. The author makes good use of government records, private papers, institutional records, and the press. Although I admire his extensive use of sources, I believe that he is too apt to take intelligence reports at face value when some of their data are certainly false.

But whatever shortcomings I have mentioned do not lead to any qualifications of my view that this is an extraordinary work on modern Indian history and the best book on the modern history of Bengal.

LEONARD A. GORDON
Brooklyn College and Columbia University

TRUONG NHU TANG. *A Vietcong Memoir*. Assisted by DAVID CHANOFF and DOAN VAN TOAI. New York: Harcourt, Brace, Jovanovich. 1985. Pp. xiv, 350. $17.95.

Accounts of the Vietnam War published in the United States have tended to take the form of a tale told by Americans about Americans. The South Vietnamese themselves, their concerns and their attitudes about the war, have rarely been given more than cursory attention. In particular, the "other side"—the leaders of the National Liberation Front and their supporters among the South Vietnamese population—appears virtually not at all.

Truong Nhu Tang's memoir is a valuable corrective to this prevailing ethnocentric view of the war. Born the son of a wealthy businessman in Saigon, the author was first attracted to the revolutionary movement as a student in France. After returning to Saigon, his leftist instincts were strengthened by the venality of the regime of Ngo Dinh Diem, which arose in the South after the Geneva Conference in 1954. For the next several years he led two lives: one as an executive of a South Vietnamese sugar corporation, the other as an active member of the revolutionary movement. In 1969 he was named minister of justice in the Provisional Revolutionary Government.

In 1975 the revolution triumphed in the South. For the author, however, victory brought not fulfillment but disillusionment. In the months following the fall of Saigon, Tang and many of his colleagues in the southern liberation movement were shouldered aside by tough and puritanical northerners who distrusted their southern compatriots because of the latter's moderate social views and their bourgeois backgrounds. In 1978 Tang followed the path of thousands of other "boat people" and fled abroad, where he eventually settled in France.

This sobering account has few heroes: not the Saigon regime, whose corruption and incompetence destroyed the hopes of those South Vietnamese who had believed that independence would bring peace and prosperity; not the Americans, who, in Tang's view, were perfectly satisfied to prop up the unsavory Saigon regime in order to achieve their own cold war interests in Southeast Asia; nor, finally, the Communist leaders in Hanoi, who manipulated naive southern intellectuals into believing that victory would lead to self-determination for the South Vietnamese people. Instead, party leaders imposed their own brand of communism, Hanoi-style, on the sullen population in the southern provinces.

Vietcong Memoir is a fascinating account of life inside the revolutionary movement in Vietnam. Tang documents his odyssey from simple patriotism to political activisim in the service of the revolution, and seldom has the mystique of the intellectual Left been so effectively portrayed. Equally valuable is the author's description of the tensions between northerners and southerners within the movement, tensions that, as Hanoi's contemporary difficulties in imposing its will on the South attest, have not been fully resolved today.

WILLIAM J. DUIKER
Pennsylvania State University

PHILIP MCMICHAEL. *Settlers and the Agrarian Question: Foundations of Capitalism in Colonial Australia*. New York: Cambridge University Press. 1984. Pp. xvi, 304. $34.50.

As Philip McMichael explicitly acknowledges, his work is an interpretation of Australian colonial

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history from the perspective of Immanuel Wallerstein's ongoing work, *The Modern World-System*. For McMichael the dominant motif is a series of conflicts among successive elites—a sheep-raising aristocracy, less well financed (and somewhat less pretentious) squatters, gold magnates, urban capitalists—each of which was integrally tied to a specific stage of Australia's incorporation as a peripheral region of the capitalist world economy controlled by Great Britain. Indeed, most of the various settler societies that were being thus peripheralized (Australia, South Africa, Argentina, the American South) showed a pronounced tendency toward political domination by classes of big landowners. But how each society faced its agrarian question was determined by how the individual conflicts worked out internally. Twentieth-century Australia is fortunate to have resolved the class struggle in favor of urban capitalism. Others were not so lucky.

Many of the pieces of Wallerstein's interpretation have been standard fare among economic historians of early modern Europe for a generation or more (as his lengthy footnotes to the *Cambridge Economic History of Europe* make clear). That is also true of the general outline of McMichael's book. Class formation and class conflict have been dominant themes of Australian historiography. And, until very recently, Australia's political relationship within a modern world system—the British empire—has probably received more than enough attention. McMichael's class analysis of Australian politics will not strike informed readers as novel.

In my view the main contribution of this book is not, as I expected, at the theoretical level but at the level of detailed analysis. In addition to reinterpreting secondary works in the light of the world-system approach, McMichael has done a good deal of admirably old-fashioned archival research in Australia. This he has put to effective use in explaining more precisely than I have seen before how John Macarthur's merino sheep fit into a specific technological phase of the British woolen industry, how the development of the London capital market enabled a new group of squatters to cut in on the gentry's monopoly, and so forth.

I have two major criticisms. First, McMichael's theoretical discussions are difficult (and sometimes quite impossible) to follow, because he tends to refer vaguely to works as a whole instead of analyzing historiographical debates concerning specific points relating to his argument. His habit of breaking up his sentences with numerous parenthetical citations is distracting. The liberal quotations from secondary works were one of the most valuable features of Wallerstein's first volume (which is why the omission of the footnotes from the paperback edition was disastrous). Following Wallerstein's practice, of course, would have made the book longer and more expensive, but the improved readability would surely have been worth it. Second, McMichael's treatment of the making of the agrarian question down to the 1850s is a good deal more satisfactory than his account of how the individual colonies faced and resolved that problem in their land legislation of the late nineteenth century, a subject that gets but one chapter of ten—even though the historiography of the later period is also considerably thinner.

Although it will not, I think, radically alter the perception of the main outline of this subject, McMichael's book is one that students of Australia, the British empire, and the Commonwealth must carefully consider.

JOHN W. CELL
Duke University

UNITED STATES

JOHN J. MCCUSKER and RUSSELL R. MENARD. *The Economy of British America, 1607–1789*. (Needs and Opportunities for Study.) Chapel Hill: University of North Carolina Press, for The Institute of Early American History and Culture, Williamsburg, Va. 1985. Pp. xxiv, 485. $35.00.

John J. McCusker and Russell R. Menard present this book as a summary designed to provoke further exploration, for they believe that colonial economic history is on the verge of a transformation. Several themes give coherence to their work: the dramatic growth of the American economy, differences in the pattern of this growth, and the interaction between external markets and internal population pressure. Central to their argument is that expansion of staple exports determined the rate of economic growth. They combine traditional and quantitative methodologies but clearly favor the new economic history.

Two questions must be asked if this approach is followed: how good are the statistical data, and how appropriate are the assumptions for the models? In America the passion for compiling statistics dates at best from the later eighteenth century; the statistics for the colonial period are skimpy, scattered, and riddled with pitfalls. A market model, moreover, postulates a "rational," profit-seeking man, who, in making decisions, has available detailed information on prices, markets, and alternative courses, all disseminated by complex communications and financial institutions. Can such a view derived from the modern world of impersonal institutions be applied to a period when such arrangements did not exist or were rudimentary? Menard and McCusker argue, for example, that slavery in the Chesapeake can be approached by assuming that planters chose their

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work force by comparing costs and output among alternative types of labor to maximize net returns. But the authors also tell us that no data exist to measure the actual supply of the various types of labor (pp. 134, 241). Is such a cost explanation necessary when a simple availability argument might account for the increased use of slaves as the supply of young rural poor from England dried up, the availability of black labor increased, more credit became available, and British merchants found additional vents for tobacco in Europe?

William T. Baxter concluded that colonial traders were often lax and careless in their business procedures and that we cannot be sure that their motive in keeping accounts was to calculate profits. McCusker and Menard tend to agree with Stuart Bruchey's rejoinder that many merchants kept good books in accordance with the best contemporary standards. Yet how adequate were those standards? McCusker and Menard's interpretation may be correct, but they acknowledge that little has been written on the subject and that studies of colonial business procedures are needed (p. 345).

Since McCusker and Menard conceive of exports as central for the entire economy, their discussion of the English acts of trade is surprising. Those laws were not, as they conclude, the culmination and epitome of mercantilism, for a significant gap existed between mercantilism and these statutes, laws that did not impose a closed trading system as writers postulated. Throughout the colonial period and after, British merchants served as middlemen in securing markets for colonial exports in Europe, although nothing in the laws excluded colonials from these functions. McCusker and Menard, who claim that British merchants were annoyed when colonists entered the traffic with the metropole, do so despite the warning of Richard Pares and others against overestimating the independence of colonial traders, for in transatlantic commerce Britons and colonists joined together as factors, correspondents, and even as partners.

Although the authors prefer to know how people behaved rather than what they thought, they have accepted conjectures about the American resentment over economic issues. Allegedly, complaints of British merchants in part led to the parliamentary legislation of the 1760s, measures introduced by the home government seemingly designed to favor rapacious Britons. If this view is correct, why did Americans after independence turn again to British merchants? Although Menard and McCusker write that a "critical description and analysis" of the economic history of the period 1783–90 "is still needed," they nevertheless conclude that this decade looked "economically much the same as the decade preceding it, in basic structure if not in detail" (p. 367).

Whether scholars approve of the approach or of the conclusions, they will find this book stimulating and of added value for the extensive list of secondary references relating to social as well as economic history.

J. M. SOSIN
University of Nebraska

JAY P. DOLAN. *The American Catholic Experience: A History from Colonial Times to the Present.* Garden City, New York: Doubleday. 1985. Pp. 504. $19.95.

Given the publication of James Hennesey's magisterial *American Catholics* (1981), one could easily quibble with the self-characterization of Jay P. Dolan's *The American Catholic Experience.* The dust jacket informs the reader that this is "the first book to reflect the new communal and social awakening which emerged from Vatican Council II" and is "*the* standard volume for this decade." Despite this unnecessary display of pretentiousness, Dolan has indeed written an important volume that is broadly comparable to Hennesey's and deserves a widespread audience.

Like Hennesey, Dolan offers the reader no overly deferential and historically skewed ecclesiastical history and focuses "on the people and not just the prelates, on the experience of religion and not just the development of the institution" (p. 10). The author wants "to look at the American experience from a new angle, from the bottom up, if you wish and write . . . a new history of Roman Catholicism in the United States" (p. 10). Dolan and Hennesey only partially succeed in writing a complete history, which can most probably be accounted for by the lack of available pre-twentieth-century scholarship and by the practical necessity of devising a manageable project for one individual in one lifetime. A truly comprehensive social history of the American Catholic experience awaits the collaborative efforts of a team of historians, social scientists, and humanists that is yet to be assembled.

Following Hennesey's lead, Dolan basically divides his history into three periods—the colonial (1500–1820), the immigrant (1820–1960), and the reform (1960–84)—with a significant concentration on the middle period. All the standard subjects are covered in a fashion both scholarly and highly readable. His last section, however, is surprisingly weak given the vast amount of readily accessible material on the post–Vatican II period. Dolan makes scant mention, for instance, of the 1976 "Call to Action" Conference during the American Catholic Bishops' Bicentennial Program, which not only set the liberal agenda of U.S. Catholicism for decades to come but also has energized and organized a movement that has since seen the bishops collec-

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tively issue statements on war and peace, on the economy, and, soon, on women.

Where Hennesey comes across as a true pluralist arguing that each age has its own attendant strengths and weaknesses, Dolan basically offers us an evolutionary interpretation clearly colored by an "Americanist" or "liberal" bias similar to that found in Andrew Greeley's *The Catholic Experience* (1967). Dolan obviously has little use for the immigrant period, which he sees as a mostly backward anomaly that delayed unnecessarily U.S. Catholicism's inevitable ascension from Plato's cave of ignorance into the light of the modern world. The author tends to aggrandize the "republican interlude" between the years 1780 and 1820 as a progressive and creative period. That perspective is eventually vindicated through Dolan's selective understanding of the meaning of Vatican II. One can reject Dolan's overall vision and claim that he has been trapped by what Owen Chadwick has referred to as the "tyranny of present opinion"—as this reviewer does—and still acknowledge the usefulness and correctness of many of his specific arguments.

JOSEPH A. VARACALLI
Nassau Community College

JONATHAN M. CHU. *Neighbors, Friends, or Madmen: The Puritan Adjustment to Quakerism in Seventeenth-Century Massachusetts*. (Contributions to the Study of Religion, number 14.) Westport, Conn.: Greenwood. 1985. Pp. xiii, 205. $27.95.

Jonathan M. Chu's book joins recently published works by Philip Gura and David Lovejoy in the study of colonial American religious radicals. Chu does not assess the morality of Massachusetts actions against Quakers; rather, he focuses on the outlook that led Massachusetts to attempt suppression of heterodoxy and on the reasons for the failure of that effort in the case of the Quakers. Because the book deals with Friends in Massachusetts before 1690, its scope is necessarily limited; Chu does not consider Plymouth or the adjustments and strife obtaining from the union of Massachusetts and Plymouth.

Chu contends that the Massachusetts reaction to Quakers was limited by its attempt to separate church and state. Consequently, he concludes, Massachusetts was unable to charge Friends with religious crimes but punished their obstreperous behavior as sedition. Thus, those four Friends executed for returning to the colony after being expelled received the death penalty for threatening state order, not for heresy. Furthermore, he states, historians' attention should be directed not to the martyrs but to those Massachusetts residents (Puritans and Quakers) who made the adjustment to each other over approximately three decades. Chu argues that Massachusetts after the Restoration left control of Friends to local authorities, whose treatment of resident Quakers varied from community to community depending on local issues and the social standing of the Quakers involved.

Chu has effectively used town records to show variations in the ways that the communities of Kittery and Salem treated Friends. He has made a courageous attempt to estimate Quaker numbers, a chancy undertaking at best since seventeenth-century Quakerism underwent several schisms and had no well-defined membership rules. He has also demonstrated that persecution of Friends declined as communities accepted Friends for what they were, not for what they might turn out to be.

In a few instances Chu's argument is unconvincing or the evidence inadequate, although nowhere do these flaws seriously detract from the thrust of the book. The author underestimates the effect of King Philip's War on Puritan-Quaker relations because of his focus on northern New England; Kittery and Salem did not suffer from that war as did Plymouth and Rhode Island. Massachusetts authorities may have prosecuted Quakers for their beliefs to a greater extent than we find in this book; indeed, the quotation from Massachusetts law suggests that lawmakers feared Quakers' beliefs as well as their civil disobedience (p. 70). These are minor points of disagreement, however, with a work that has significantly altered conclusions about early dissent in the Bay Colony.

ARTHUR J. WORRALL
Colorado State University

ANN UHRY ABRAMS. *The Valiant Hero: Benjamin West and Grand-Style History Painting*. (New Directions in American Art, number 1.) Washington, D.C.: Smithsonian Institution. 1985. Pp. 254. Cloth $39.95, paper $19.95.

This book offers a stunning display of interdisciplinary scholarship. Ann Uhry Abrams wisely concentrates on narrative pictures that Benjamin West painted in his native Pennsylvania, in Italy, and in England during the first stage of his career there, when he served as court artist for George III, for it was then that he developed his basic formula for the Grand Style of history painting. Since this formula combined techniques, settings, themes, and imagery from sources as diverse as ancient history, classical sculpture, eighteenth-century theater, and Georgian politics, the author had no choice but to adopt an interdisciplinary approach. Moreover, she succeeds in reevaluating a few myths that West cleverly planted in the biography that John Galt wrote for him, and she fashions one of her own chapters into

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an impressive psychograph by "reading" the self-portraits of West, who, as a writer, had no skills whatsoever.

Of all the paintings discussed in Abrams's book, six benefit particularly from her interdisciplinary point of view. *The Death of Socrates* (1756) reveals West's familiarity with the theater (even though he was a Quaker) and his indebtedness to William Smith, who, like Socrates, was a champion of free speech. *The Savage Chief* (ca. 1761) shows that West could not resist advertising his stay in Italy by modeling an Indian on the Apollo Belvedere. *The Choice of Hercules* (1764), through action rather than symbols, reflects the indecision felt not only by West but also by George III as they reached crossroads in their lives. *Agrippina Landing at Brundisium with the Ashes of Germanicus* (1768) and *The Departure of Regulus from Rome* (1769) served as patriotic allegories for the benefit of West's contemporaries and as prime examples of his borrowings from the eighteenth-century theater's improved lighting and peripheral seating. The famous *Death of General Wolfe* (1770) qualifies as revolutionary, "not because the supporting cast of officers were dressed in contemporary clothing but because Wolfe himself was depicted as an ordinary mortal" (p. 180). Readers will be startled to learn that West probably charged soldiers to paint their likenesses into this picture, that here and in similar tableaux he did not hesitate to move the scene of death to a more dramatic site, and that, as long as he could achieve verisimilitude, he included among the bystanders only those whom he thought deserved to be there, not those who had actually witnessed the death.

The book is amply illustrated, but the format is too small for some of the multi-figured paintings and for cartoons with balloon-enclosed text. Although *The Death of Socrates* is called a "landmark of early American art" (p. 58), it does not appear among the seven color plates. Still, this is an impressive study of West as visual historian.

JAMES T. CALLOW
University of Detroit

DAVID W. ROBSON. *Educating Republicans: The College in the Era of the American Revolution, 1750–1800.* (Contributions to the Study of American Education, number 15.) Westport, Conn.: Greenwood. 1985. Pp. xvi, 272. $35.00.

David W. Robson has examined the relationship between politics and higher education before, during, and after the American Revolution. Prior to the imperial crises of the 1760s, Robson argues, American colleges were implicitly political. Colonial educators and theorists understood that the character of higher education and the character of the state were intimately associated, but the idea of civic education—education for state service—tended to receive only "formulaic" expression (p. 13). Political knowledge came indirectly to most students, as a by-product of their reading of the classics and of the political lessons that "lurked within" the curriculum (p. 15). With the onset of the Anglo-American crisis and the growth of the independence movement, these earlier tendencies were transformed. Political lessons became purposeful and specific. A "new generation" (p. 33) of faculty members, for the most part steeped in the writings of the radical Whigs or Commonwealthmen, became active participants in the resistance to British policies. Students, observing these actions of their instructors and absorbing the political messages (now explicit) contained in lectures and readings, eagerly supported the patriot cause. American colleges, politicized, were thus the "agents of revolution" (p. 251). The Revolutionary War itself intensified the American commitment to radical Whig ideology, and the revolutionary colleges, in spite of the dislocations of war, quickly developed a common patriotic front. Ideologically inspired, American educators championed the new republic and undertook curricular reforms intended to perpetuate its existence. Such unity in the collegiate community, however, proved to be short-lived. No sooner was the war over than American colleges succumbed to the social, political, and cultural differences that agitated the citizenry of the new nation. Politicization, begun some two decades earlier, culminated in partisan competition. By the early nineteenth century, these divisions within the body politic had resulted in the severing of the connection between American colleges and American political culture. "That dissociation," Robson concludes, "marked the end of an era" (p. 252).

Robson has carefully and systematically assembled data for all of the revolutionary colleges. Robson's observations, however, are not likely to surprise most historians. That developments within the colleges corresponded to developments in society at large, that radical Whig ideas found a receptive audience among faculty and students, and that factional strife in the early republic affected the tone, if not the structure, of the collegiate curricula are not unanticipated findings. Furthermore, Robson's argument concerning the causal sequence in the politicization of the collegiate community—the faculty "absorb[ing] and then impart[ing] to their students the Commonwealth Whig political ideology" (p. 251), an argument incorporated into the very arrangement of the chapters in the first half of the book—is perhaps too narrowly focused and, indeed, is contradicted by some of his own evidence. The problem is made manifest in the case of the College of William and Mary where, Robson contends, the replacement of "Tory" faculty members

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with "Patriots" in 1777 successfully "politicized students according to the Commonwealth Whig ideology" (p. 109). From Robson's earlier discussion of student activities, however, we know that by 1777 the William and Mary students were already so Whiggish that the "Tory" faculty "could not enforce discipline" within the college (p. 105). The politicization of these young Virginians must therefore be traced to sources "outside the classroom" (p. 82). This last observation is crucial because what it suggests is that such institutional indices as curricular changes and library holdings, which constitute the bulk of Robson's data, cannot adequately account for the politicization of the revolutionary colleges. The processes of education in general, and the acquisition of political knowledge in particular, must be more broadly defined.

MELVIN YAZAWA
University of New Mexico

JONATHAN R. DULL. *A Diplomatic History of the American Revolution*. New Haven: Yale University Press. 1985. Pp. xii, 229. $15.95.

Jonathan R. Dull places the diplomacy of the American Revolution in the setting of the late eighteenth-century European balance-of-power system. The author declares that it is time for a new synthesis placing emphasis on all the participants in the war for American independence "and even [on] some of the onlookers, such as Russia" (p. xi). The net result is enriching but adds little of major importance to earlier studies. The story has already been told fully and with great authority by both Samuel Flagg Bemis and William C. Stinchcombe.

The author draws on recent monographic studies of European diplomacy, and he is to be complimented for his thoroughness. But his findings, interesting and ably presented, do not add up to his conclusion that the study of Samuel Flagg Bemis "is by now vastly out of date" (p. 179). That book remains a classic in spite of its nationalistic tone and Bemis's exaggerated distrust of Vergennes.

Dull presents a reasonable and significant conjecture. He states, "Although it cannot be proven, it seems likely that the appeal of American independence to Vergennes lay chiefly in its probable effect on the European balance of power" (p. 59). He cites French concern with the new strength of Russia in Eastern Europe and Crimea as evidence, but, as he himself acknowledges, the conjecture lies beyond absolute proof.

The author finds that until 1778 and the conclusion of the alliance with France "the impact of the American revolution on continental Europe was slight" (p. 66). Aside from Spain's entry into the alliance with France and Spain's driving the British out of Florida, this did not change. As to the other European rulers, they "expressed an interest in the American Revolution, but it is difficult to see any concrete changes in their diplomatic policy as a result" (p. 71).

Dull devotes four chapters to relations with France. He notes that popular belief has fixed on the idea that the American victory at Saratoga was responsible for the French decision to enter the war. France had for months considered that the proper time for entering the alliance would come in the late winter of 1778, and the final decision did not immediately follow the reports of the victory at Saratoga. Serious negotiations did not begin until January 8, 1779. It also seems likely that Vergennes's concern that the British and Americans were about to reach an agreement was less than genuine but that he enlarged on it to persuade Louis XVI, who was reluctant to go to war. Vergennes's purpose in expressing concern was probably to convince Spain to enter an alliance. The author's account of Vergennes's many considerations in deciding that the right time had arrived when France could safely go to war contributes to our understanding of the European side. This is a valuable contribution.

PAUL A. VARG
EMERITUS
Michigan State University

PAUL DAVID NELSON. *Anthony Wayne: Soldier of the Early Republic*. Bloomington: Indiana University Press. 1985. Pp. x, 368. $27.50.

With the possible exception of Light-Horse Harry Lee, few military leaders of the early republic excite the imagination as much as Anthony Wayne. Called "Mad Anthony" by his men for his unfailing enthusiasm for action, Wayne commanded Pennsylvania and Continental troops in virtually every theater of combat during the Revolutionary War. He was among the last officers to leave active military duty. Sent south to assist Nathan Greene in South Carolina and Georgia after Cornwallis's surrender at Yorktown, Wayne sought out new opportunities to engage the British army long after other commanders had resolved to leave the terms of peace to the diplomats gathered in Paris. Despite a debilitating illness contracted in the Georgia swamps during the summer of 1782, Wayne remained in the field through the reoccupation of Charleston in early 1783. When he left South Carolina for home in July 1783, Wayne carried with him a well-earned reputation as a stern disciplinarian and an outspoken advocate for the military. A decade later, while serving as commander of the Legion of the United States on the northwest frontier, Wayne proved

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again a demanding officer devoted to his troops and eager for action.

Paul David Nelson has written a lively account of the career of this soldier whose name dances in and out of the republic's early military history. In what is a thoroughly researched and gracefully written biography, Nelson has placed Wayne in the political as well as military history of the revolutionary era. Wayne, initially an enthusiast of the militia and the Old Whig values implied, became an advocate of a strong regular army after militia troops under his command at Ticonderoga mutinied in early 1777. Similarly, the general separated himself from Pennsylvania's radical political leadership early in the war, rejecting the democratic implications of the unicameral government created in his home state in 1776. By 1780, Wayne had come to believe that independence depended on the officers and men of the regular army and that a republic would be undermined by "mob rule" unless founded on a strong civilian government dominated by a few "respectable" citizens. Those attitudes shaped his activities in a variety of political arenas after the war. Wayne backed state constitutional reform as a member of Pennsylvania's Council of Censors, advocated legislation on behalf of Revolutionary War veterans while serving in the state assembly, voted for the Constitution at Pennsylvania's ratification convention in 1787, and supported a strong regular military establishment during his brief tenure as a congressman from Georgia.

Military and political service, Nelson suggests, brought Wayne the honor and glory on which his personality thrived. It also cost him a close personal relationship with his wife and family. While focusing tightly on Wayne's public career, Nelson has, nevertheless, provided a fascinating glimpse of the Revolutionary War's impact on the life of an officer's family. Mary Penrose Wayne lost her husband to the call to arms. Virtually ignored by Wayne, she reared their two children and successfully managed the family farm through the war, enduring both Wayne's much-publicized philandering and his ill-conceived and disastrous rice and land speculation in Georgia during the 1780s.

Nelson has presented a multidimensional portrait of a masterful military officer. Students of American military history and of the early republic will profit from it.

LAWRENCE DELBERT CRESS
Texas A&M University

DAVID F. LONG. *Ready to Hazard: A Biography of Commodore William Bainbridge, 1774–1833*. (IMPrint Series.) Hanover: University Press of New England, for the University of New Hampshire, Durham. 1981. Pp. xii, 359.

In naval history classes at the U.S. Naval Academy, we used to call him "Hard-Luck Will Bainbridge." As the sole commanding officer in the history of the U.S. Navy compelled three times to haul down his colors, he certainly seemed entitled to the nickname. In the quasi-war with France (1798–1800) his vessel was the only U.S. warship taken by the enemy. In 1800 the dey of Algiers compelled him to hoist Algerine colors on his frigate and haul passengers, animals, and cargo to Constantinople. In the war with Tripoli (1801–05) he surrendered his frigate to the Tripolitans, who held him and his crew prisoner for nineteen months until the U.S. government paid their ransom.

As David F. Long points out, however, Bainbridge, despite undeniable hard luck, was also a fortunate man. All three surrenders resulted from his own recklessness, yet they cost him neither rank nor reputation. In 1798 he lost the eighteen-gun *Retaliation* when he ventured close to two warships without ascertaining that they were heavily armed enemy frigates. Two years later he incautiously moored his frigate *George Washington* so near an Algerine fortress that she could have been shot to pieces had he resisted the dey's demands. In 1803 he sent away his accompanying schooner, which could have protected his frigate *Philadelphia* when she ran aground in Tripoli harbor, and was surrounded by Tripolitan gunboats. During Bainbridge's months of captivity, however, Americans forgot their exasperation at his surrendering his ship and welcomed him home as a national hero. In the War of 1812 he crowned his curiously unblemished reputation when, in command of the USS *Constitution*, he defeated HM frigate *Java*.

The writing in the first half of this book, which includes the scenes of action, is vivid and racy. There is some letdown in the postwar chapters, reflecting Bainbridge's tiresome complaints and demands based on his overestimate of his own abilities. The commodore's personality does not come through clearly, but that may be because of its many paradoxes. As Long points out, "He was at various times in his life, and sometimes simultaneously, astute and short-sighted, brave and timid, kind and cruel, frank and devious, good natured and cantankerous" (p. 272).

Long is to be commended for his meticulous and far-ranging quest for authenticity. Aware of the tendency of naval history to accumulate legends—and no period more so than early American naval history—he has confronted his sources with appropriate skepticism and tirelessly compared one against another in search of facts. Accurate and generally readable, *Ready to Hazard* deserves more attention than it has received.

E. B. POTTER
U.S. Naval Academy

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NEIL LONGLEY YORK. *Mechanical Metamorphosis: Technological Change in Revolutionary America*. Foreword by BROOKE HINDLE. (Contributions in American Studies, number 78.) Westport, Conn.: Greenwood. 1985. Pp. xviii, 240. $35.00.

Neil Longley York has taken John Kasson's speculations about the marriage of technology and republicanism and subjected them to a more thorough empirical investigation. He does not challenge Kasson; indeed, York argues that the American Revolution was a quest for technological as well as political independence. "During the 1780s American appreciation for and identification with technology took shape, setting the stage for changes in the next century that propelled the nation into the industrial age" (p. 175).

York's book consists of a series of topics rather than an integrated investigation of technological enthusiasm. It begins with the clamor for home manufactures, because Americans resented their dependence on English goods. The failure to achieve self-sufficiency, particularly in munitions production, spurred Americans to promote manufacturing in the interests of the republic.

But the link between invention and manufacturing was not quickly discerned. Scientific societies remained blissfully ignorant of the innovations that were revolutionizing British industry. The Continental Congress spurned inventions—notably, David Bushnell's Turtle, Stephen Sayre's gunnery system, and Joseph Belton's musket—that might have challenged England's military superiority. Even in areas of colonial expertise, the classic example being the Pennsylvania rifle, American technology proved to be a failure because Washington could not or would not devise battle plans to maximize American advantages. In at least one instance colonials did innovate: American engineers devised effective *chevaux-de-frise* (river obstructions) to protect Philadelphia from naval attack.

Only in the postwar decade did an American elite begin to attach significance to invention and relate it to mechanization. Cognizant of the changes within English industry and determined to make the republican experiment work, they discussed the need for American manufacturing. They established a patent system—one measure of their interest in innovation and their recognition that it deserved pecuniary reward.

This volume combines prodigious research with a firm grasp of colonial technology. The essays that focus on inventors, engineers, and inventions sparkle. Yet the author promises more than the material warrants. For example, in the chapter entitled "The Inventor Celebrated," York describes our most able inventors—James Rumsey, John Fitch, and Oliver Evans—and how they became progressively embittered by the patent system and by their failure to achieve either public or financial recognition. The rationale for this seemingly inaccurate title comes in the concluding paragraphs, where York observes that these men "became heroic citizens, at least in their own eyes and in the eyes of later generations" (p. 206). Similarly, the central thesis of this monograph relies on utterances of a small but powerful elite. Their commitment to a "more complex technological order" did not result in broad popular support or even minimal government aid. At best, this period witnesses the tentative origins of our technological enthusiasm.

These observations should not dissuade historians from consulting this fascinating volume. It sketches virtually every major American inventor and invention, and it offers a range of hypotheses about the factors promoting and limiting technological change. For those interested in the era or the field, this book is an outstanding introduction.

DIANE LINDSTROM
University of Wisconsin

CYNTHIA OWEN PHILIP. *Robert Fulton: A Biography*. New York: Franklin Watts. 1985. Pp. xii, 371. $18.95.

Robert Fulton has been well served by Cynthia Owen Philip. Fulton emerges as a mechanical genius whose fertile, inventive mind was continually devising and perfecting new technologies. He dazzled Europe with his submarine, but he was never able to gain the French government's support for a full-scale development of his terrible new weapon of war. Eventually turning his attention to steamboats, Fulton successfully demonstrated one on the Seine in 1803. After an unpleasant interlude in England when he attempted to demonstrate the utility of his torpedoes, Fulton returned to the United States to build a steamboat for service on the Hudson River. He succeeded in 1807, and for the remainder of his life he perfected and improved his growing fleet of steamboats and fought avaricious competitors who challenged his patent as well as his and Robert R. Livingston's monopoly.

Fulton's tremendous energy and ambition is amply demonstrated. One of the strengths of this biography is that it shows the trials and tribulations of an inventor-entrepreneur in the early republic. Fulton fought tenaciously for monopolies, government contracts, and his vision of dominating the steamboat traffic not only on the East Coast but also on western waters. Once Fulton had demonstrated a workable steamboat, however, a host of imitators appeared. By the time of his death in 1815, rivals such as John Stevens, Aaron Ogden, Oliver Evans, Henry Shreve, Nicholas Roosevelt, and William

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Thornton were challenging his preeminence in the field, and his monopolies were being ignored or contested in courts and legislatures.

Fulton appears here as a fascinating and talented but not altogether admirable character. An artist, mechanic, inventor, engineer, and entrepreneur, this son of a tailor rose to become a wealthy man and a gentleman who made a significant contribution to the advancement of engineering and technology in his country. Unlike earlier Fulton biographers, Philip does not skirt the delicate question of Fulton's sexual preferences. She presents a persuasive case that Fulton was bisexual, and, contradicting the opinion of James Woodress, Joel Barlow's chief biographer, she leaves little doubt that Fulton was a lover of both Joel and Ruth Barlow in a ménage à trois.

The research for this book is obvious, but the documentation is skimpy. The book is based heavily on primary sources, but, strangely enough, Philip does not appear to have consulted the most recent biography of Fulton by John S. Morgan. There are occasional lapses. William Jones is twice cited as the secretary of war, rather than as the secretary of the navy, and she persists in referring to "Elihu" Whitney. These and other minor errors, however, do not detract from an otherwise excellent biography.

C. EDWARD SKEEN
Memphis State University

MICHAEL A. LOFARO, editor. *Davy Crockett: The Man, the Legend, the Legacy, 1786–1986*. Knoxville: University of Tennessee Press. 1985. Pp. xxiii, 203. $14.95.

Although its title seems to suggest a comprehensive treatment of the history and the myth of Davy Crockett, this book is a collection of articles on particular aspects of Crockett mythography and historiography. The book is valuable for its description and analysis of materials that have not previously received critical or historical attention as well as for the care and sophistication with which the contributors revise precedent scholarship and raise and resolve a range of theoretical issues.

Five of the eight articles deal specifically with new or neglected aspects of the Crockett "canon." Michael A. Lofaro's treatment of the Crockett almanacs originating in Nashville provides a detailed look at a local variation of the Crockett myth-making industry and offers some interesting contrasts with versions of the hero later adopted in other locales and in more nationally oriented media. It will be of great interest to students of popular culture and local or regional cultural history. Other pieces investigate Crockett's place in the national popular culture in a variety of media over the course of more than a century. Richard Hauck discusses stage melodramas; Normal Bruce analyzes a newly discovered silent film about Crockett; Margaret King gives an excellent account of the Disney version; and Charles Wolfe's essay on Crockett in popular song traces the hero's progress from minstrel shows to television. These essays might provide a good starting point for the investigation of one of the most interesting aspects of popular culture mythography in general (and the Crockett myth in particular): why and how is it that certain historically embedded myths like that of Crockett survive the times that generated them and continually recrudesce in our culture?

This question is raised directly in the article by Catherine Albanese, who connects the Crockett myths with the Indo-European tradition of the "wild man." She is concerned with developing an understanding of the factors that shape the *longue durée* in cultural history, and her integration of the methods of social history, the anthropology of religion, and archetypal criticism makes her contribution exceptionally interesting from a theoretical point of view. Hauck's discussion, "Fact and Fiction in the Crockett Story," does a nice job of describing the historiographic problem that confronts the student of myth and ideology; he also offers useful critiques and revisions of previous scholarship. John Seelye attempts to untangle folklore from fakelore, and his essay is especially interesting for its treatment of modern folklorists as contributors to (as well as analysts of) the folklore/fakelore complex.

The juxtaposition of Seelye and Hauck throws into relief a hidden but fundamental similarity between the historiographer and the anthropologist or folklorist: both confront, in different guises, the same question of "authenticity." The historian measures the representation of the hero against the verifiable facts of the case, but he or she must also confront the power of the fiction to reflect and to shape the mentality of the society being studied. Folklorists accept a priori the importance of fiction as data, but they require a test that will allow them to distinguish between fictions that are the productions of a "folk" or a community and those that are generated by an institutionally distinct commercial popular culture. This collection reflects that convergence of concerns and methods between the disciplines, which is one of the most promising tendencies in the profession.

This is an important and useful addition to Crockett scholarship and the discussion of cultural historiography, although it does not provide a comprehensive treatment of its subject. As a supplement to and revision of prior scholarship by Walter Blair and Richard Dorson, it is extremely valuable.

RICHARD SLOTKIN
Wesleyan University

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PAGE PUTNAM MILLER. *A Claim to New Roles*. (ATLA Monograph, number 22.) Metuchen, N.J.: Scarecrow, for The American Theological Library Association. 1985. Pp. ix, 241. $17.50.

This dissertation has been published as yet another number in the American Theological Library Association monograph series. As befits the first work of a good historian, the study provides more data than it does analytical perspective. Page Putnam Miller concentrates on the contributions of some Presbyterian women during the first decades of the nineteenth century.

An initial chapter focuses on biographical data concerning three women: Sarah Clarkson Ralston (1766–1820), a stalwart member of Second Presbyterian Church, Philadelphia; Susan C. Spotswood Bott (1774–1853), an early member of the Tabb Street Presbyterian Church, Petersburg, Virginia; and Helen Davis Beatty (1802–76), who established the Steubenville Female Seminary with her husband Charles, then pastor of the First Presbyterian Church of that community. Subsequent chapters are devoted to Presbyterian policies concerning women, to a portrait of motherhood in antebellum Reformed life, to the work of women in voluntary associations, to their leadership in Sabbath and Sunday schools, and to an introduction to female education.

Much of the material can be located elsewhere, but this gathering of diverse sources aids the student of the topic. In addition, obvious care has been given to the selection of the data, so the reader can form opinions and learn easily of the contributions of women to American religious life. Miller also includes an introduction, asserting that she has extensive profiles of almost two hundred Presbyterian women. Sadly, she does not devote more than a few paragraphs to analysis of the overall data that she has compiled. She has not profiled more than a dozen women, even in succinct fashion, throughout the text. Furthermore, Miller does not aid the reader by offering the location and quality of her data so that others can glean wisdom from it.

In addition, she seldom situates the various enterprises and "roles" of Presbyterian women in a wider setting. Did their confession of Reformed faith distinguish them from other antebellum women? Were there differences of opinion among Presbyterian women based on region or matters socioeconomic? Did wives of clergy, several of whom she mentions, differ in perspective from other women? This reader hopes Miller and others will pursue these and other related questions in subsequent efforts.

LOUIS WEEKS
Louisville Presbyterian Theological Seminary

KATE H. WINTER. *Marietta Holley: Life with "Josiah Allen's Wife"*. (New York State Study.) Syracuse: Syracuse University Press. 1984. Pp. x, 182. $28.00.

Today few people have ever heard of Marietta Holley, but her impact on shaping public attitudes toward women's issues during the late nineteenth century was perhaps as significant as that of Frances Willard, Elizabeth Cady Stanton, or Susan B. Anthony. Not a professional reformer or a political activitist, Holley instead made her mark as a "local color" humorist in the manner of Mark Twain, Josh Billings, and Artemus Ward. Her numerous stories and twenty-one novels about "Samantha," also known as "Josiah Allen's Wife," helped raise her readers' consciousness concerning the necessity for temperance and female suffrage. At the same time, Holley commented on the sad state of American race relations, on the hypocrisy of churches, and on the absurdity of the "cult of true womanhood." What was unique about Holley's work was that its humor carefully sought to persuade her readers to accept her point of view rather than to alienate them. Consequently, Holley gained many supporters for social reform; she also alerted the nation to the hardships regularly endured by farm wives.

Holley understood all too well the lives about which she wrote. Born in northern New York in 1836, she belonged to a family of small farmers who had to struggle to survive. In her youth, Holley possessed a passion for writing, an activity in which she engaged only furtively at first. A spinster, Holley spent almost her entire life on her family's farm. She rarely left it except for occasional trips to Saratoga and New York City. Holley exemplified the retiring Victorian lady; as her fame grew, she consistently refused to participate in any of the public activities to which she was regularly invited.

Since she never spoke in public and deliberately burned her private papers, Holley provides numerous challenges to her biographer. For this volume, Kate H. Winter relies on Holley's disjointed published reminiscenses as well as on miscellaneous correspondence, newspaper reports of her activities, and formal histories of the North Country. The results are impressive. Winter not only pieces together Holley's life story but also interweaves it with material from Holley's "Samantha" stories to show how, over time, Holley and her fictional character became one.

The book's major failing comes from a lack of perspective; Winter's narrow focus can become tiresome. She needs to compare Holley's life and attitudes with those of her contemporaries. In spite of this weakness, Winter provides a useful study for historians and literary scholars. More important, she

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has saved a major feminist literary figure from oblivion.

MARIAN E. STROBEL
Furman University

ROBERT W. FRAZER. *Forts and Supplies: The Role of the Army in the Economy of the Southwest, 1846–1861*. Albuquerque: University of New Mexico Press. 1983. Pp. x, 253. $22.50.

In this book Robert W. Frazer argues that in the fifteen years before the Civil War "the army was the single most significant factor in the economic development of the Southwest" (p. ix). By building forts and restraining hostile Indians in the area from Taos to El Paso and from Arizona eastward to the Texas border, the army stimulated new agricultural efforts by Spanish-Americans while attracting Anglo-American homesteaders, ranchers, and miners to New Mexico. Using an impressive number of statistics, based heavily on army records, the author shows that the population of New Mexico increased more than 50 percent in these years while corn, wheat, beans, cattle production, and jobs doubled. New mines were opened, and service facilities, such as hotels and flour mills, increased in number.

Statistics about the number of soldiers in the Southwest and the amount of foodstuffs, hay, lumber, and adobe brick used by the army also point up the limited resources of the region and sometimes the even more limited capabilities of the stubborn and often unwise army commanders who set policy. Frazer is especially frank about Colonel Edwin Vose Sumner's poor choice of locations for forts and his obsession with economy. In general, none of the commanders chalked up an impressive record of accomplishments.

Frazer has concentrated so exclusively on the world of the military that most of the important territorial officials and influential merchants of New Mexico remain shadowy figures. He narrows the context further by ignoring the role of the Santa Fe trade—reputed to be worth one million dollars in the 1840s—as a stimulant to the New Mexican economy, as well as the lively trade in mules and horses from Chihuahua or California. Again reflecting the views of the military, the local population is criticized for not providing adequate supplies, lumber, hard currency, and buildings for the army and for charging all that the market would bear. Why a conquered people should feel obligated to provide these services is a question that seems not to have occurred to the army or to the author.

In describing the wagon routes laid out from San Antonio to El Paso in 1849, the author stresses the accomplishments of the military, but the merchants of San Antonio and a number of civilians were also key figures in pioneering the upper and lower routes. This is perhaps understandable, since the bibliography of this book concentrates so heavily on military sources.

Certainly the United States army did play a major role in the economic development of the Southwest, but other factors helped as well. Although the army may have accounted for the bulk of federal funds spent there, federal largess was not exclusively military. And, as Frazer himself notes, the army in New Mexico, far from wanting to aid the economy, tried to bypass the local markets by turning soldiers into farmers so that they could raise their own vegetables and other foodstuffs; that failing, the army imported as many goods as possible from the states. Frazer's argument is basically sound, but it would have been stronger had it been put in a wider economic and political context.

HOWARD R. LAMAR
Yale University

DREW GILPIN FAUST. *James Henry Hammond and the Old South: A Design for Mastery*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1982. Pp. xviii, 407. $8.95.

James Henry Hammond (1807–64) is best remembered for his "Cotton is King" speech in the United States Senate in 1858. What was unusual about him? It was certainly not his humble background, his large plantation (which he gained through marriage), his modest literary efforts, his agricultural reform, nor his political career. His service in the United States House of Representatives (1835–36), South Carolina governorship (1842–44), and the United States Senate (1857–60) was, in each case, brief and undistinguished.

But Hammond's inordinate ambition, personality, complicated character, and almost obsessive record keeping make him a fascinating person for serious study. With rich materials at hand and a clear understanding of the political scene in antebellum South Carolina, Drew Gilpin Faust has written a splendid account of Hammond's life and times.

The author's subtitle, *A Design for Mastery*, is the focus of her study. Southern planters, Hammond once explained, "are accustomed . . . to control and scorn to be controlled" (p. 307). This driven man grew up under a stern father, succeeded at South Carolina College, and was determined to dominate all around him—his siblings, wife, children, slaves, poorer neighbors, and, to an extent, political colleagues.

Hammond's dogged efforts caused him anxiety, frustration, ill health, and little satisfaction. The Carolina chivalry never quite accepted him; the poor did not pay him proper deference; his brother

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Marcellus became a drunkard; his brother John, in a final act of defiance, served as a Union army surgeon in the Civil War; his sons chafed under his constant direction and criticism; his slaves skillfully evaded absolute control. Faust's chapter "In Search of Despotic Sway" is a superb account of master-slave relationships on a Carolina plantation.

Hammond lacked the self-control that mastery demanded. Instead, he complained that others did not understand him, that his craving was for love. But did he know how to return love? He never gave his children proper credit; his excessive fondling of his Hampton nieces was certainly not love and created a scandal that almost destroyed him politically. His liaison with two slave women caused his wife to desert their loveless marriage for many months. Later, living in luxury, Catherine Hammond sadly remarked, "With every thing to make us happy there are few families that are less so" (p. 330).

In his political career Hammond was a capable speaker and pamphleteer; he catered to democracy while fearing democracy's lack of order. He was a radical in his early years yet gradually became a cautious supporter of unified southern action in line with John Calhoun's stance. He hoped to inherit Calhoun's mantle but failed to win election to the Senate in December 1850.

Feeling "annihilated forever," he retired to his Silver Bluff plantation until the call finally came in 1857. But Hammond was tardy in joining the secessionist bandwagon, and the Confederacy bypassed him. He died young, an embittered man.

Faust seems somewhat uncertain how to assess Hammond's place in history. Nevertheless, this fine biography deserves the laurels it has been awarded.

ERNEST M. LANDER, JR.
Clemson University

J. WILLIAM HARRIS. *Plain Folk and Gentry in a Slave Society: White Liberty and Black Slavery in Augusta's Hinterlands*. Middletown, Conn.: Wesleyan University Press. 1985. Pp. xv, 274. $25.95.

The late Frank L. Owsley would no doubt be immensely pleased to know that the southern yeomen farmers, at long last, are receiving new attention and coming into clearer focus. This short study by J. William Harris makes a large contribution to that process. Harris's quite readable (but highly quantified) volume fully and vividly reveals the interaction of yeomen, large and small slaveowners, and blacks in a manner that few historians have rivaled. The area covered consists of those counties in Georgia and South Carolina for which Augusta served as the chief cotton and wholesale market, and the time span is from the 1850s until the end of the Civil War.

After courteous nods to those historians who have emphasized the alleged hegemony of the great planters, Harris explains early on that he finds himself more in agreement with those, like U. B. Phillips and George Frederickson, who emphasize white agreement on fundamentals, only one of which was profound racism. "The common experience of landownership and, equally significant, the common participation in the commercial cotton market," he argues, "gave planters and farmers similar economic values" (p. 6). Yet white unity remained precarious, for, although black slavery was believed to guarantee white liberty, it also stood as a dramatic reminder that political equality did not mean economic equality. Aware of discontent among both nonslaveholders and slaves, planters could never be sure that the vaunted white solidarity would be strong enough to withstand attacks from increasingly powerful antislavery Republicans. (One may still doubt, however, whether their ambiguous combination of racism and antislavery could have prospered in the South, where a large black population was a reality unknown to most of the antebellum North.)

While whites in the Augusta area proudly but apprehensively guarded their cherished liberty, the blacks, as Harris convincingly delineates them in a chapter entitled "The Underlife of Slavery," lived under a "kind of unacknowledged negotiated agreement" whereby a partly autonomous slave community emerged—a community that Harris portrays as extending beyond the plantation quarters and farm boundaries to become "a kind of wider, underground spiritual and social community" (p. 43). With only one-fourth of the slaves living on plantations having fifty or more slaves, the remaining slaves were almost equally divided between farms having fewer than twenty slaves and plantations with between twenty and fifty slaves. Thus, the interests of owners ended up matching the desires of slaves to ensure that slaves belonging to different masters married, often worked together, and regularly worshipped together as well as joined in corn-shucking parties and other such communal activities. A lively underground market involved stolen goods exchanged for liquor. The upshot of all this, Harris insists, was that slaves rejected the masters' proclaimed patriarchy and, "in the cracks of the peculiar institution, . . . found room to define their predicament in their own terms" (p. 61).

In the chapter "Portents of Class Division," Harris mines the rich manuscript collections of James Henry Hammond, Alexander Stephens, and Linton Stephens, among others, to show the strains and tensions between white classes, yet both statistics and the careers of individuals are used to portray a

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significant social fluidity in the Augusta area. Moreover, farmers and planters exchanged visits, tools, and money, while also, and most important, participating in the intensely real communities of their evangelical congregations and the courthouse-centered politics of the era. Harris sums up the matter nicely: "True, slaveholders were, on the whole, *of* their communities; indeed, they were leaders of their communities. But equally true, the community was their master. They could rule only by consent. . . . If the mass of small farmers—their community—turned against them *as slaveholders*, then slavery would be finished" (p. 122).

After following his Georgians and South Carolinians through the traumas of secession and war, Harris suggests an ironic conclusion: the Confederate war effort itself "produced the 'military despotism,' class divisions, and slave resistance that masters had gone to war to prevent," and the "resulting internal conflicts were a principal cause of the final collapse of the Confederacy" (p. 191).

ROBERT F. DURDEN
Duke University

PAUL D. ESCOTT. *Many Excellent People: Power and Privilege in North Carolina, 1850–1900*. (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1985. Pp. xxii, 344. $29.00.

When Jonathan Worth died in 1869, someone referred to him as "a model of the . . . gentlemen of the old school." Paul D. Escott undoubtedly would agree with that assessment, since he uses Worth as a perfect example of what Escott has labeled "the elite" of North Carolina. His basic argument is that the state was—and still is—dominated by a relatively small group that has consistently disregarded and trampled on the fundamentally democratic character of the people of the state—the yeomen farmers, the workers, and the blacks. But Escott's elite are not necessarily "gentlemen"; most of them are entrepreneurs of the type who created the New South, men clearly motivated more by profits than by their love of humanity.

Escott's basic goal was "to analyze both changes and continuities in social attitudes, social structure, and power" (p. xvii). This he does very well, working from an extensive assortment of primary sources, especially county records, newspapers, and collections of individual papers. This thorough examination convinced him that relationships between classes did not change much between 1850 and 1900. Nor did democracy advance in those years. In fact, as a result of the fusion politics of the 1890s, the state was less democratic in 1900 than it had been in 1880. The elite had employed racial fears to entrench themselves more deeply as a ruling class. Escott speaks often of the relationship between race and class and argues that class, "the dominant partner" (p. xvii), was more important than race in holding the power structure together.

In separate chapters Escott examines the major events of the period and devotes a chapter to each important social group—capitalists, blacks, yeomen, and workers. In every instance his conclusions are those described above. The bottom rail never was on top and had little chance of being placed there. The elitists, who receive more attention than any other group, consistently controlled the state's economy and its governmental machinery. The Democratic party was the primary villain, whereas the Republicans served the interests of the blacks and, presumably, a significant number of the lower-class whites.

One of the most significant aspects of Escott's book is his revisionist stance on the development of the textile industry. According to Escott, "W. J. Cash was badly mistaken" (p. 222), and "the real story is very different from Broadus Mitchell's myth" (p. 213). According to the earlier writers, the workers were not class conscious and were even grateful to the employers for providing jobs. But Escott argues that the workers were both class conscious and resentful, forced into atrocious working conditions by economic necessity.

The book is seminal rather than definitive. It challenges the old interpretations and raises numerous questions that will have to be answered by future students. Is it possible, for example, that a majority of ordinary North Carolinians were inherently conservative and had no interest in overthrowing their paternalistic oppressors? Why do their descendants send Jesse Helms to the United States Senate? If "many excellent people" had the vote, why did they not choose leaders from among themselves? Is it possible that Escott's elite ruled by assent rather than by the manipulation of power?

RICHARD L. ZUBER
Wake Forest University

ORVILLE VERNON BURTON. *In My Father's House Are Many Mansions: Family and Community in Edgefield, South Carolina*. (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1985. Pp. xxi, 480. $29.95.

In this book, Orville Vernon Burton aims "to construct as much as possible a total, almost encyclopedic, history of nineteenth-century Edgefield [County, South Carolina] families and their communities" (p. xvii). That brief statement of purpose identifies the work's strengths as well as its shortcomings. This book is an ambitious saga that chronicles social relationships among and between blacks and whites

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in the course of several generations. Yet it tends to sprawl over the Piedmont landscape, treating many topics in anecdotal fashion, without penetrating to the core of nineteenth-century politics and economy—the forces that shaped those relationships.

In an effort to present Edgefield's "total history" (p. 4), Burton seems to have examined virtually every available shred of literary and statistical data related to household structure, family life, and religious practices. He discusses the antebellum period from the perspective of white elites (drawing on their personal papers and plantation records) and of slaves (using the Federal Writers' Project slave narratives). The Freedmen's Bureau archives shed light on the turbulent era of Reconstruction. Burton concentrates his efforts, however, on analyzing manuscript census data for 1850, 1860, 1870, and 1880 in order to make detailed comparisons between white and free black families before the Civil War and white and freed black families after the conflict. The result of this massive quantitative study (based on contingency analysis) is an impressive array of tables and charts surveying the characteristics of all mid-nineteenth-century Edgefield households, regardless of race, geographical location (town or country), and socioeconomic class.

Because Burton views blacks and whites as members of a single community, he remains sensitive to the values and life experiences that united them. For example, he highlights the central role of religion among all people and argues that, "in cultural values and in the structure of the family, blacks and whites had much in common" (p. 230). Wealth was a major determinant of household composition for both races. Indeed, one of Burton's significant contributions to Reconstruction historiography is his discussion of the postwar Afro-American family, which was usually headed by a man, especially in rural areas. Burton speculates that the larger proportion of black, female-headed households in towns gave rise to the myth of the black matriarchy. This crucial distinction between rural and urban black households holds true for other regions of the late nineteenth-century South as well. (In his introduction, Burton makes a neat case for two propositions: that Edgefield was unique by virtue of its large number of "state and national trouble makers" who were outspoken in their defense of slavery and, at the same time, that the county was representative of the South's peculiar racial and class dynamics.)

The author divides broadly defined chapters into (for the most part) short sections that cover a variety of topics. Unfortunately, he often presents nonstatistical evidence in the form of anecdotes, and this approach hinders systematic development of crucial themes. For example, the section on divorce, premarital sexuality, and adultery (in chapter 3, "The White Family and Antebellum Social Structure") begins with the not-too-revealing statement that "family relationships were not always tranquil" (p. 136) and then goes on to catalogue interesting but rather disconnected examples of family scandal among whites. Another topic, the impact of slavery on the white family, is dispatched in less than three pages.

Burton's book provides a wealth of information about intimate relationships between men and women, masters and slaves, landowners and laborers; curiously enough for a study about the land of southern "fire-eaters," politics (traditionally defined) is absent. Burton alludes to the underlying chain of white hegemony that binds the New South to the slave South. Nevertheless, the men who controlled the region's political economy are for the most part portrayed as husbands, sons, brothers, and church congregants in the context of specific incidents. How their influence as planters, politicians, and patriarchs evolved out of the crucible of war awaits scrutiny in the next "total history" of Edgefield.

JACQUELINE JONES
Wellesley College

JACQUELINE JONES. *Labor of Love, Labor of Sorrow: Black Women, Work, and the Family from Slavery to the Present*. New York: Basic. 1985. Pp. xiii, 432. $25.95.

This is a superb and richly interpretive history of black working women in America, based on extensive and careful use of printed sources. Jacqueline Jones proceeds from the thesis that black people, regardless of class, experienced historical events in a way fundamentally different than did whites and that black women's historical experience differed significantly from that of men of their race and from that of white women of their class: "Black women actually inhabited a unique subculture, one not shared by either black men or white women" (p. 5). One of the strengths of Jones's work is that she analyzes and delineates that subculture with great sensitivity as to its complexities and with a fine balance between larger generalizations and concrete detail.

From slavery days onward, black women's work encompassed two realms: that of their own families and communities, in which they earned respect and honor, and that of the paid labor force, in which, because of race discrimination, they were forced into domestic service and into the lowest-paid, most despised jobs—often, at hard physical labor. Whites tended to see black mothers' responsibilities to their own families as detrimental to the interest of whites. The constant tension between these two conflicting interests defined the life and work experiences of black women in a unique way. Jones reconstructs their lives, struggles, and attitudes from slavery to

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the present, skillfully synthesizing material from printed primary sources and secondary works and making good use of statistical data. She shows us "not only what [black women] did, but also what they desired for themselves and their children" (p. 5).

Jones succeeds admirably in depicting black women, not only as victims of discrimination and exploitation but also as agents for social change, defending their own and their families' rights, defining cultural priorities for their communities, and struggling to mediate between Afro-American and white culture. No matter how degraded the position of black women in the work force may have been at any given time, their greatest priority always was the educational improvement of their children. They ceaselessly worked toward that end, which became for them the very definition of racial advancement, within their own families and communities and, when possible, within the political realm. The remarkable story of the community and institution building of black working women is slighted in the narrative, probably because of Jones's dependence on published sources. Manuscript sources offer abundant evidence of these activities, and they must be explored before a definitive and complete history of black women can be written.

Jones is at her best in her hard-hitting analysis of the differences in the ways black and white women encountered sexism in the post–World War II period. Her description of the different ways that women of the two races are structured into the economy is balanced by her careful attention to the long history of attempts at coalition building. Jones's study offers a fine synthesis of the best available knowledge about black women workers. It provides a readable and dramatic account of their historical struggles, experiences, self-perceptions and values. Her work should be of equal interest to historians in the fields of Afro-American, labor, and women's history and to those wishing to enrich the traditional survey courses.

GERDA LERNER
University of Wisconsin

JAMES MACGREGOR BURNS. *The American Experiment.* Volume 2, *The Workshop of Democracy.* New York: Alfred A. Knopf. 1985. Pp. xi, 671. $24.95.

This second volume of James MacGregor Burns's three-volume history of the United States begins with the Emancipation Proclamation in 1863 and ends with the Bonus March episode in the summer of 1932. The final volume will therefore begin with Franklin Roosevelt, of whom Burns's two-volume biography was a prize-winning achievement, perhaps Burns's finest work. *The Workshop of Democracy* reads just as well or better, but for historians it presents some problems.

The text and notes occupy 638 pages, divided into sixteen chapters organized in five chronological parts: the close of the Civil War and Reconstruction, the late nineteenth century through the election of 1896, Progressivism through 1912, the Wilson years, and the decade 1921–32. As the periodization suggests, much of the space is devoted to the activities of presidents and, to a lesser extent, of Congress, the Supreme Court, and the military (in 1864–65, 1898, and 1917–18, as well as Indian wars and Caribbean and East Asian operations of the "Progressive" era). Burns also writes about capitalists such as John D. Rockefeller, Sr., Henry Ford, and Andrew Mellon; reformers such as Helen Hunt Jackson, Booker T. Washington and W. E. B. Du Bois, Jane Addams and Florence Kelley, Herbert Croly, and Walter Lippmann. But slightly less prominent figures are absent, despite the author's stated aim of treating "second and third cadres" of leaders. Burns states his principles of organization and selection in the acknowledgements: "to emphasize the role of purposeful leadership in the processes of historical causation," including "economic as well as intellectual and political leadership," while also describing something of the lives of the common men and women of this democratic society (p. 639). The problem is that, although he writes at length about the leaders (especially those in national politics who are already familiar to textbook readers and classroom lecturers) and often about farmers, workers, immigrants, women, blacks, and other almost reified groups, he oscillates between them and the leaders without ever quite analyzing their relationships or coming to grips with the dynamics of social change.

Most disappointing is the absence of the findings and thrust of the "new social," "new economic," or "new political" history of the past fifteen years. With few exceptions his sources are printed—both primary and, more often, secondary. The latter are sometimes up to date but are seldom complete on any subject. On Reconstruction, for example, he cites recent work by Leon Litwack, James McPherson, and Les Benedict, which is fine, but has not benefited from the work of, for example, W. C. Harris, Charles Flynn, or Steven Hahn. On immigration he cites Irving Howe (and for a moment even sounds like Howe) but omits the books of Josef Barton, Caroline Golab, John Briggs, Dino Cinel, and John Bodnar, not to mention important dissertations such as those of Judith Laird (Kansas, 1975) and June Alexander (Minnesota, 1980). There is scant trace of these historians' efforts to examine the immigrants' lives in terms of their own intentions and their European origins. Black migration from the rural South to the urban North appears in the

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context of demand for factory workers in World War I rather than in the causal complex of southern as well as northern conditions. Demographic matters such as birth rates, death rates, and changing family size are not Burns's concern. Public health and public works are mentioned rarely and not systematically. The author, in sum, has avoided much of the recent historiography.

One refreshing area is the book's attention, though incomplete, to women's history. As for a new overarching interpretation, however, there is none; indeed, the book does not absorb, much less surpass, the organizational synthesis offered by Robert Wiebe (1967) and others. For what it includes, and what it omits, it resembles a discursive, more than ordinarily entertaining, and interpretively predictable, recapitulation of the material in the more successful textbooks of the 1960s. As a general history of America, this is not an innovative work in a class, say, with that of Daniel Boorstin. It does present a broad narrative of familiar economic, intellectual, and, especially, national political events. Thus, it is an obsolete book, beautifully written.

WALTER NUGENT
University of Notre Dame

ROBERT J. KACZOROWSKI. *The Politics of Judicial Interpretation: The Federal Courts, Department of Justice, and Civil Rights, 1866–1876*. (New York University School of Law Linden Studies in Legal History.) New York: Oceana. 1985. Pp. xiv, 241. $32.50.

Robert J. Kaczorowski revises and extends the rich corpus of recent scholarship on Reconstruction-era constitutionalism by focusing on the perspectives and actions of the national legal establishment. The Thirteenth, Fourteenth, and Fifteenth Amendments led to "revolutionary constitutional and legal theories" (p. xi). The vigorous and positive response of U.S. attorneys such as Benjamin Bristow in Kentucky, federal inferior court judges in a wide range of decisions, and the attorney general's office, at least at first, established "a virtually unlimited national authority over civil rights," brushing aside ambiguities and existing contrary directions in the law (p. 1). Their efforts, at the outset of the Reconstruction era, had "a far more revolutionary impact on American constitutionalism than scholars have appreciated" (p. xiii).

Unfortunately, the vigorous reach of this new theory exceeded the ability of the system to handle the problems it posed or to resist the power of the political backlash to it that quickly emerged. "It is ironic," Kaczorowski suggests, "that the legal difficulties federal officers encountered in enforcing civil rights stemmed less from having too little authority than from having been given more authority than they knew how to apply " (p. 122). The newly created Department of Justice, a potential bulwark for this new national authority, never had the resources to meet its new commitments, and intractable questions of definitions about jurisdiction in criminal matters remained between the state and the federal authorities. This confusion and administrative limitations helped undermine the new judicial authority. It was, Kaczorowski writes, "the Attorney General's decision to reduce judicial expenditures" that crippled the nationalizing trend and not any "changing judicial theories or the inability of federal legal officers" (p. 107). In such a situation potent local resistance to civil rights and bitter political debate over federal power intensified and ultimately shifted the judicial direction back toward state power doctrines, a trend definitely reestablished in the Slaughterhouse and Cruikshank cases in the 1870s. In short, the theory was there and was adequately and vigorously articulated. Its failure to take hold said more about community resistance, political realities, and personal commitments than it did about the failure to establish a persuasive intellectual claim to national legal jurisdiction in criminal cases.

Kaczorowski has skillfully worked through the lower court decisions, briefs, official and personal correspondence, administrative records, and newspapers to form and texture his argument. Much of his work resonates with familiar historiographic themes and some previous understandings. His contribution is to mix the elements differently, provide much additional information from underused sources, and give a fresh perspective about the emerging nationalizing legal theory and its advocates. If an occasional misspelling jars and a misstep or two is taken in interpretating the political, as against the legal-constitutional, world, such flaws detract little from an interesting argument and well-handled explication of a critical aspect shaping a still lively and contentious era.

JOEL H. SILBEY
Cornell University

HARRIET E. AMOS. *Cotton City: Urban Development in Antebellum Mobile*. University: University of Alabama Press. 1985. Pp. xvi, 311. $29.50.

In 1972, David L. Smiley chided colleagues for fixing their scholarship on "planter, plantation, staple crop, and the Negro, all set in a rural scene." Since that time, scholars have broadened their research perspectives to include, among other topics, the urban South. The results of this work have been not to diminish the importance of Smiley's quartet for antebellum southern history but rather to place these elements into the context of local, state, and

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regional events of which they were a part. As J. Mills Thornton III and Harry L. Watson have demonstrated in their work on Alabama and on Cumberland County, North Carolina, respectively, antebellum southerners were hardly the precapitalist individuals in Eugene Genovese's rendition of the Old South; rather, they aggressively pursued economic development that, in turn, held serious political and social ramifications for their localities and beyond.

Harriet E. Amos's book on antebellum Mobile offers detailed, well-documented support for the Old South's stridently capitalist orientation, a bent tinged with the tragedy of unfulfilled dreams. Amos's purpose is twofold: to explain the city's growth and response to it and to delineate Mobile's relationship to a national economy centered in New York as well as the economic and political impact of this connection. The author's treatment of the city's growth, especially after 1820, is unremarkable and formulaic, involving a catalogue of services that the city provided, growth of local government, and building an urban infrastructure. Amos's major contribution occurs in her analysis of the city's interregional connections.

Cotton dominated Mobile—the staple accounted for 99 percent of the city's export trade—and New York dominated the cotton trade. As early as 1825, agents, ships, credit, and insurance floated down from Gotham, securing Mobile as a colonial dependency, however beneficial such arrangements became to local merchants. New Yorkers siphoned from 40 to 75 percent of the cotton trade profits from Mobile. Northerners held positions of political and economic leadership within the city and were instrumental in establishing a public school system, undertaking civic beautification projects, and promoting railroads.

The results of such dependence included the inability to accumulate a sufficient capital base to engage in internal improvements and in industrial and commercial enterprises. This ensured continued subservience, as well as a growing frustration and intolerance, especially after the panic of 1857, that produced an increasing insularity and militancy in political affairs. At this point, Amos's analysis falters: a more explicit connection with Thornton's work would have placed Mobile's situation in a statewide context. First, it is unclear how a city that voted for moderation in November 1860 turned so abruptly for secession two months later: why did no voices emerge to point out the economic benefits of union to counter the claims of those who perceived an independent and wealthy city within a southern confederacy? Second, to what extent did a badly divided, predominantly rural state view Mobile's cotton moguls as potential threats to republican institutions? Nevertheless, the book remains a solid contribution to the growing literature on the centrality of economic development for antebellum southerners, as well as the important role occupied by southern cities in such development.

DAVID R. GOLDFIELD
University of North Carolina, Charlotte

COLLEEN MORSE ELLIOTT and LOUISE ARMSTRONG MOXLEY, editors. *The Tennessee Civil War Veterans Questionnaires*. In five volumes. Compiled by GUSTAVUS W. DYER and JOHN TROTWOOD MOORE. Easley, S.C.: Southern Historical Press. 1985. Pp. xxi, 432; xxi, 433–873; xxi, 875–1315; xxi, 1316–1786; xxi, 1787–2264. $250.00.

These five volumes contain the typescripts of 1,650 questionnaires sent to Tennessee's Civil War veterans beginning in 1914 and returned to the Tennessee Historical Commission by 1922. Although two slightly different forms were used, the questions asked of all respondents were virtually identical, falling into several general categories: basic information about the wealth, occupation, and daily activities of veterans' families before the Civil War; the quality of relationships between slaveholders and nonslaveholders; economic and educational opportunities for antebellum whites; prevailing attitudes about work; wartime experiences; postwar careers; and genealogy. Given the paucity of sources from small slaveholders, yeoman farmers, and propertyless whites in the Old South, these volumes are a welcome publication. If they are not quite a mother lode, the documents can nevertheless be interesting and sometimes useful.

As with the more widely known Works Project Administration interviews of former slaves, the veterans' questionnaires present certain problems, which can, however, be overcome. Even assuming the reliability of octogenarian memories, the respondents often admitted that they were "too young to remember" many things about the antebellum South (p. 162). Subjective evaluations of class relations and economic opportunities in the antebellum South may have been influenced by experiences in the very different society of the early twentieth century. Certainly the questionnaire itself reflects the thinking of its authors' time. Only one of some forty-five questions addressed postwar experiences; only one asked about women and the family. Perhaps most treacherous was the compilers' determination to prove the social harmony of white society by asking questions that confused personal relations with class relations.

Thus, the safest conclusions that can be drawn are too often trivial: the rich had the best homes and the most education; they owned the best land and the

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most slaves, had the most domestic servants, and were least likely to do field work; their children were most likely to prosper and hold public office. By contrast, the majority of farmers and their wives worked hard. Not surprisingly, army life is portrayed as being difficult, often brutal, sometimes exhilarating. Few readers will be struck by the fact that poor men were most likely to sense elitism, that rich men were most likely to confine their associations to "respectable" elements, or that the overwhelming majority recalled generally friendly and equitable relations between slaveholders and nonslaveholders. Alas, such findings tell us little we did not already know.

But the documents have potential virtues as well. A systematic examination of the interviews with one hundred fifty Unionists may prove especially useful. The professions of cordial interclass relations must be balanced against the substantial testimony, from rich and poor alike, that slaveholders and nonslaveholders often did not live in the same areas and that the wealthiest planters frequently kept to themselves. Particularly interesting are the nearly uniform attacks on laziness and the widespread allegiance to meritocracy. If aristocratic values more substantial than mere snobbery infused antebellum society or if an ethic of leisure held sway in the Old South, the memory was all but obliterated by the time Tennessee's veterans filled out these questionnaires.

JAMES OAKES
Princeton University

THOMAS F. GOSSETT. Uncle Tom's Cabin *and American Culture*. Dallas, Tex.: Southern Methodist University Press. 1985. Pp. xi, 484. $29.95.

Thomas F. Gossett, whose *Race: The History of an Idea in America* was published in 1964, here turns his attention to Harriet Beecher Stowe and her major work. The six chapters that rehearse Stowe's life contribute little new for historians, but Gossett deals expertly with the composition and major themes of *Uncle Tom's Cabin*. On the one hand, he reminds us of Uncle Tom's youthful strength, his refusal to betray his fellows even under torture, and his redemptive, Christ-like death. On the other, he explores the ambiguities of the self-freed mulatto slaves who all chose Africa over America, of Uncle Tom's rationalized acquiescence in being sold South, and of Yankee characters more racist than southrons. Less familiar themes—the central role of family and the ways in which community identity shaped characters' political and social views—are convincingly developed.

Gossett's freshest material—his treatment of antebellum reactions to the publication and theatrical performances of the novel—unfortunately lacks the systematic analysis that he gives to Stowe's work itself. Instead, he treats northern, southern, and European critiques as separate entities, each one a pastiche of essentially unrelated individual comments. The result is a thinly textured chronology rather than a thematic exploration.

Therein lies the book's central weakness. Despite its title, Gossett adds little to our understanding of American culture. The author, who is no anthropologist, understands culture as the manifestation of national themes through popular literature. Yet his failure to develop such a theme erodes his book's meaning. Indeed, the thirty-two pages of Willie Lee Rose's 1978 Harmsworth lecture, *Race and Region in American Historical Fiction*, tell more about the relation of *Uncle Tom's Cabin* to American culture than Gossett's four hundred pages. More than a "book turned drama," Stowe's work was a national and a public event both reflecting and feeding a new cultural paradigm soon to lead to civil war as well as a private reaffirmation of "family and . . . the force of cultural continuity" (Rose, p. 30).

Instead of building a similar model for the interaction his title implies, Gossett trudges diligently through the literary and theatrical variations on the novel and the criticism they engendered. He tells us, though ignoring the coincidence, that in the same decade in which D. W. Griffith filmed *The Birth of a Nation*, based on *The Clansmen*, Thomas R. Dixon's equally racist best seller, white critics of Uncle Tom denounced him as an unbelievable gloss on Negro inferiority while potential black critics remained prudently mute. Again without linking the two, Gossett reports that the generation that acclaimed Alex Haley's *Roots* also made "Uncle Tom" a term of reprobation signifying a weak-willed passivity substantially at odds with Stowe's original character.

Gossett traces this transmutation through many literary sources without connecting it to the larger context of American culture. In so doing, he makes *Uncle Tom's Cabin* little more than a lightning rod through which divergent racial attitudes have struck American ground.

JANE H. PEASE
University of Maine

ALAN AXELROD, editor. *The Colonial Revival in America*. New York: W. W. Norton, for the Henry Francis du Pont Winterthur Museum, Winterthur, Delaware. 1985. Pp. x, 377. $29.95.

A century after its birth, the United States celebrated its past by beginning a colonial revival that lasted well into the first half of the twentieth century. With the passing of yet another century, scholars are increasingly examining the revival itself.

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Derived from papers presented at two recent conferences held at the Winterthur Museum, this book edited by Alan Axelrod contains fourteen essays that underscore the richness of this revival. Ranging widely in scope, the essays analyze the revival's artistic, socioeconomic, and intellectual dimensions. Similarly diverse are the authors, who include an editor, museum officials, and academicians from different disciplines. Fortunately, the essays themselves do not fluctuate widely in terms of quality; most are quite good and some are superior.

The art, architecture, and material artifacts of the colonial revival provide the primary focus for the book. Karal Ann Marling presents an interesting look at Grant Wood's perceptions of the colonial past, and the architects Wilson Eyre of Philadelphia and Neil Reid from Georgia receive critical attention from Edward Teitelman and Catherine M. Howett respectively. Mardges Bacon examines the close links between the revivification of colonial architecture and the powerful Ecole des Beaux-Arts, whose imprimatur for both method and style many American architects found desirable or necessary. Susan Prendergast Schoelwer convincingly argues that, the Parisian influence notwithstanding, it was the World's Columbian Exposition held in Chicago in 1893 that secured popularity for the revival. Before the exposition many of its twenty million visitors had probably seen little genuine colonial architecture, decorations, furniture, costumes, or other memorabilia. Profound alterations in critical and popular tastes resulted from this exposure.

The revival extended well beyond the arts. Additionally, it represented a response to the dramatic changes in American life dating from the late nineteenth century. With industrialism and immigration threatening familiar patterns and traditions, worried Americans extolled the virtues, real and imagined, of their past. William B. Rhoads notes in his essay that colonial architecture proved useful for acculturating Old World immigrants to New World values. Less sanguine in tone is William Butler's depiction of the restoration of Litchfield, Connecticut, as an example of both an elite, idealized version of the past and a bulwark against encroachment by alien elements. Beverly Seaton examines popular historical novels penned between 1890 and 1910, finding themes and characters that reinforced the cultural hegemony of a WASP elite. Relatedly, Celia Betsky claims in her persuasive essay that literary and artistic aspects of the revival helped soften and make more acceptable the jarring impact of modernism on American life.

The legacy of the nineteenth-century revival seems perdurable. Americans continue to exhibit a pronounced taste for colonial-style homes and silverware and, as visiting palmers, trek annually in large numbers to such meccas as Colonial Williamsburg, Historic Deerfield, and Old Sturbridge Village. By critically examining the revival's range, diversity, and implications, this book adds a useful dimension to this compelling heritage.

ROBERT MUCCIGROSSO
Brooklyn College
City University of New York

MACDONALD SMITH MOORE. *Yankee Blues: Musical Culture and American Identity*. Bloomington: Indiana University Press. 1985. Pp. 213. $29.50.

Although this is a short book, it is no more felicitous for that, primarily because of Macdonald Smith Moore's unchecked tendency to inflate both the importance of his subject and the length of his work with pompous, pretentious language (after a while, I gave up counting the number of times "elide" appears), gratuitous sociological jargon, obscurity, and mystification. Here is a representative sample: "They [Yankee composers] balanced the claims of formalism and functionalism and held both in uneasy symbiosis with a mediating idealist and imaginative naturalism" (p. 7). Say *what*?

The Yankees of Moore's title are a group of composers born of Anglo-Saxon, Protestant stock in New England during the 1870s (and therefore sometimes referred to as "centennial"). The thesis of the book, as nearly as I can determine, is that these men hoped to use the music they wrote to reassert the cultural and ideological dominance of their tribe in the nation's affairs. Although Moore lists seven such composers, only two of them, Daniel Gregory Mason and Charles Ives, receive more than passing mention; my uncharitable suspicion is that Ives, who all but vanishes from sight in the second half of the narrative (only three of the forty-eight citations to him in the index are to pages following the midpoint of the book), is more or less dragged in by the hair because he is by far the best known of the "centennial" cohort. Without Ives to provide window dressing, in other words, Moore would have nothing more than a book about Mason, who, though a widely published critic and the head of the music department at Columbia University during the 1920s, is now entirely forgotten—and deservedly so, to judge by the details Moore recounts.

Where Ives was seemingly obsessed with demonstrating that composition was a "manly," as opposed to an "effeminate" (that is, homosexual) art—hence his rejection of the music of Debussy, for example, as "voluptuous," "senuous," and "slimy" (p. 55)—Mason's fixation was on the spiritual pollution that was invading this country's music. The sources of this pollution were Negroes, with their hedonistic jazz; avant-garde European composers (Igor Stravinsky was evidently the chief offender), whose

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mechanistic rhythms mirrored this soulless urban-industrial age and thus aided in the further destruction of wholesome life in the U.S.; and, above (or below) all, such Jewish composers as George Gershwin, Aaron Copland, and Ernest Bloch, who served as middlemen in catalyzing the travesty. To counter these dreadful developments, Yankee composers such as Mason during the 1930s promoted the Oklahoma-born Protestant composer Roy Harris as their Great White Hope; but, before his triumph could be realized, "virtually overnight the Second World War cut short discussions about a musical 'white hope'" (p. 168). This last assertion is simply proclaimed, not demonstrated. And in this abrupt, inconclusive fashion the narrative comes screeching to a halt.

What significance does this all have? In his introductory chapter Moore writes that "musical culture" in the twentieth century "came to be regarded [by whom?] as a renascent American civil religion" and that, "because Yankee composers had managed to link music in the United States with issues of American identity, music criticism became [for whom?] a primary locus of national cultural conflict in the 1920s" (p. 3). These propositions may or may not be true. As my questions indicate, I find them dubious; in any event, Moore has supplied no evidence to support them. No doubt, Yankee composers, or some of them at any rate, were passionately involved in these issues, but to conclude that all their polemical exchanges comprised more than a tempest in a teapot, especially during the Great Depression of the 1930s, is rather another matter. What the argument needs is proof. What Moore gives us instead are postulates.

Incidentally, the title of Madison Grant's notorious tract is *The Passing of the Great*—not "White"—*Race* (p. 185 n. 39); the name of the renowned Ellingtonian trombonist is Tricky—not "Trick"—Sam Nanton (p. 112); and the dictum that "jazz went public in 1917" (p. 183) with the first New York performances of the all-white Original Dixieland Jazz Band certainly requires further explication if the author does not wish to be charged with racism.

FRANK KOFSKY
California State University, Sacramento

PAUL K. CONKIN. *Gone with the Ivy: A Biography of Vanderbilt University*. Assisted by HENRY LEE SWINT and PATRICIA S. MILETICH. Knoxville: University of Tennessee Press. 1985. Pp. xiii, 810. $49.50.

Paul K. Conkin has written a solid, detailed, and lengthy history of Vanderbilt University, from the first attempts to found it in the 1850s right up to the 1980s. The book is very much an "authorized" account, and Conkin was allowed to draw on a surprisingly wide array of university sources, including the most recent minutes of the Board of Trust and the active files in the chancellor's office. But Conkin does not hesitate to criticize aspects of Vanderbilt's past or to note its current weaknesses.

When Conkin began work on this book, he inherited an enormous mass of material collected by Vanderbilt history professor Henry Lee Swint. For that reason, Swint is listed on the title page. But this large resource may have played a not entirely beneficial role and is perhaps the reason that this account is so extremely detailed. At times, it seems that every change of dean, every new professor, will be mentioned. Conkin himself appears to have become weary. Long stretches of the writing are pedestrian and cliché-ridden. Materials are "not worth the paper they are written on" (p. 333); one chancellor "had to play it tough" (p. 139), while another nearly "blew his top" (p. 396) or "drew an ace" (p. 405). Whenever a school or department is in trouble, it "flounders." Words are repeated several times within a sentence or two. One wonders if Conkin, like Mark Twain, was in too much of a hurry to make it short. There are, in addition, far too many typographical errors.

That the book has these failings is unfortunate, for it is in general a first-rate university history, far superior to most entries in the field. When Conkin turns to a subject that interests him, such as the Fugitives and the Agrarians, these lapses disappear. He is clear, concise, graceful, and interesting.

Gone With the Ivy will not cause scholars to change their views about the development of higher education in the United States, but it will certainly provide welcome additional detail for anyone working in the field. Conkin did not write, however, with that larger field in mind. His audience, as he makes clear in his eloquent postscript, is the people of Vanderbilt—its board, administrators, faculty, staff, alumni, and perhaps even students. He wants them to understand why a university needs productive research scholars. He wants Vanderbilt to avoid the temptation to imitate Harvard and Yale and to "nourish . . . its own distinctive traditions" (p. 744). To be able to achieve that end, he points out, the people of Vanderbilt must have understanding, which can only come from "historical self-consciousness," the goal to which he has dedicated his book. It is a worthy objective, and one can only hope that Conkin's audience is attentive, for in these pages there is much that those associated with this important national institution can learn about its strengths and weaknesses.

BROOKS MATHER KELLEY
Yale University

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RICHARD A. WINES. *Fertilizer in America: From Waste Recycling to Resource Exploitation*. Philadelphia: Temple University Press. 1985. Pp. vii, 247. $34.95.

In this book Richard A. Wines surveys in competent fashion the emergence of the fertilizer industry in the United States from the early 1800s to about 1880 when it had assumed its modern character. Based on wide research in manuscript and printed sources, this comprehensive study of an important but hitherto largely unexplored topic will be of value primarily to agricultural, environmental, and business historians.

The fertilizer industry, according to Wines, originated near New York, Philadelphia, and Baltimore. In those areas, thin soils, declining fertility, and growing markets combined with the availability of urban wastes to make possible a recycling system in which farmers sought to return to the land nutrients taken from it by the growing of crops. This recycling of urban wastes was little more than an extension of the increasingly common practice of applying farm-produced fertilizers to land, but it also introduced agriculturists to the use of nonfarm additives.

Guano, the dried excrement of birds first found in commercial quantities on the Chincha Islands off Peru in the 1840s, was the material that converted farmers to modern fertilizers. Because Peruvian guano was rich in nitrogen, its application produced dramatic results, at least in the short run. A guano mania ensued, characterized by the fertilizer's wide acceptance by farmers, particularly in the Chesapeake Bay area, and by a vigorous search for new supplies that at times had distinctly imperialistic overtones. Later sources of nitrogen included dried blood, other animal wastes, and cottonseed, but all these sources of nitrogen remained relatively expensive for decades.

Those guanos that were deposited under humid climatic conditions were useful primarily for their phosphorus content. That plant nutrient, however, proved to be more easily available through the use of superphosphates, first produced by treating ground bone with sulfuric acid. Later, phosphate rock in South Carolina, Florida, and Tennessee became the primary source.

Potassium, the third of the basic plant nutrients, was provided in the early days by materials such as wood ash. After the Civil War, the discovery of huge deposits of potash in Germany gave that country practically a monopoly. Much of the material came to the United States in the form of kainite, a low-grade salt of potassium, or of muriate of potash.

By 1880, numerous companies, in addition to the firms that had once sold guano, were busily manufacturing and marketing complete fertilizers containing the basic plant foods in various quantities and ratios. The industry was seeking to achieve stability by eliminating fraud, and about half of the states had established offices to analyze and certify the contents of fertilizers. Use of fertilizers was common in the East Coast states and in the lower South as far west as Alabama. It was in the latter area, which had thin soils, extensive leaching of plant nutrients, and low humus content and which possessed the distinctive characteristics of post–Civil War agriculture, that commercial fertilizers would be most important until well into the twentieth century.

ROY V. SCOTT
Mississippi State University

WILLIAM G. ROBBINS. *American Forestry: A History of National, State, and Private Cooperation*. Lincoln: University of Nebraska Press. 1985. Pp. xv, 344.

Forestry as a profession is less than a century old in America. Despite its brief passage, it has enjoyed a generous accommodation by archivists and librarians that has spawned a spate of historical works since 1945. A United States Forest Service contract provided William G. Robbins opportunity to add another volume of forest history to his earlier work, *Lumberjacks and Legislators* (1982).

Because foresters deal in long-term planning of a crop that takes many years to grow, protect, and bring to multiple uses, their natural instinct is to keep careful record of success and failure. In this the forester is the likely ally of the historian; indeed, foresters have increasingly turned to the historian for research and writing skills as well as for analysis. Robbins serves both professions well.

The origins of forestry in America are briefly traced in the opening chapter, which gives special attention to the early programs of cooperation between the newly created profession (most of whose members were employed by federal agencies until after World War I) and forest and rangeland users in the private sector. Succeeding chapters disclose how repeated demands for federal regulation were voiced by concerned citizens and how such demands were debated within the forestry profession and in public forums. Cooperative programs have been addressed in the main to material conditions surrounding the forest products trade, and Robbins rightly points out that the greatest benefits have been reaped by those who controlled massive stumpage and who, through their trade associations, carried on sophisticated lobbying activities in Washington and most state capitals. Cooperative programs for the small woodlot owner have been much trumpeted but have had far less significant results. Robbins provides much evidence that "forest products leaders have been largely successful in convincing legislators that federal support for private policy

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would serve the national and public interest" (p. 263) and that federal foresters, operating at all levels, have more often than not been supportive of such belief.

Not all readers will be pleased by the lengthy consideration of cooperative programs relating to fire prevention and fire fighting and the comparatively brief attention given to programs that have addressed other problems of great concern to foresters: insect and disease control, range improvement, fish and game management, wildlife and wilderness preservation, forest taxation, and urban forestry. Some will question Robbins's further aggrandizement of Bernhard Eduard Fernow, Gifford Pinchot, Edward Tyson Allen, and William Buckhout Greeley (who is incorrectly cited in the index as William G. Greeley) while giving only passing note or no note at all to such important participants as Overton Price, Herman H. Chapman, Benton MacKaye, Albert F. Potter, David T. Mason, Samuel Trask Dana, and Aldo S. Leopold. But such criticisms only reflect the complexity of the subject and the multiplicity of documentary and other source materials now available to scholars. Those who follow Robbins down the trail of national, state, and private cooperation in forestry will discover many opportunities to enlarge our knowledge.

ELWOOD R. MAUNDER
Aptos, California

WILLIAM D. ROWLEY. *U.S. Forest Service Grazing and Rangelands: A History*. (Environmental History Series, number 8.) College Station: Texas A&M University Press. 1985. Pp. xiv, 270. $29.50.

The history of the U.S. Forest Service and its range management is not exactly unexplored. Even so, scholarly investigations of the constantly changing history of this agency continues to accrue. For this and other reasons, a new and updated account is welcome. Partly a synthesis of previous work but mostly original research, this narrative history, written by William D. Rowley, covers everything of importance from the formation of the national forests in 1891 through the Wilderness Act of 1964 and on past 1980. The account also includes quite a bit of technical detail on forestry. The interpretations of the author are mostly based on common sense, and his book is, and will continue to be, a reliable and comprehensive reference work.

For the most part, the narrative concerns the western branch of the federal government, not just the Forest Service. On the one hand, Rowley's is a tale of conflicts but seldom of bloodshed, a story of the painfully slow acceptance by stockmen of governmental regulation of federal land. On the other hand, the story revolves around the persistent efforts of the Forest Service to apply science to the problems of conservation on the western range. In so doing, Rowley makes sense of the record, particularly about the Forest Service's efforts at plant succession.

Oddly, no one seems to have had evil or devious motives. Such a presentation is probably proper, because the pursuit of motive is a dangerous enterprise in writing history. Rowley does show that the Forest Service was constantly at the center of political controversy, as was the Division of Grazing of the Bureau of Land Management after 1934. The conflicts and the cooperation of the two agencies are handled fairly, although the author tends to favor the Forest Service a little.

As the years passed, the political power of the stockmen gradually diminished. The rise of urban areas and the appearance of the new environmentalists (both related to population increase and the urbanization of America) continued to reduce the strength of local interests. The Forest Service had little to do with causing these changes, although it did benefit by gaining greater control over grazing in the National Forests during and after the 1960s.

The events of the 1970s and 1980s are treated more briefly and, as always, dispassionately. Rowley's succinct account of these decades shows what the rest of the book might have been. Not all the details were needed for the account of the changes and the direction of the changes; the western stock industry (chiefly, cattle and sheep) has always been more important for those engaged in it than for the rest of the nation. Despite the resistance of the stockmen, the use of the land for recreation and aesthetics slowly became more important nationally. The author tells this part of the tale in a quiet but effective way. His account, overall, is both objective and complete.

These virtues will probably infuriate the partisans of this or that cause. Still, it will be a sturdy reference work for years to come. The footnotes are at the bottom of the page, where they belong, and the bibliography is a good start.

JOHN T. SCHLEBECKER
Smithsonian Institution

ALLAN M. BRANDT. *No Magic Bullet: A Social History of Venereal Disease in the United States since 1880*. New York: Oxford University Press. 1985. Pp. viii, 245. $19.95.

Venereal diseases evoke a sense of repugnance in the general public, as Allan M. Brandt thoroughly documents. Unfortunately, it seems to have evoked a similar response among professional historians, for *No Magic Bullet* is the first comprehensive study

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of the subject in many years. As such, it is a welcome addition to the growing literature related to sex in American history.

Brandt traces the tangled web of venereal disease policy, from the Progressive era through Thomas Parran's New Deal fight against the spread of venereal diseases and the military hygiene program during the Second World War. This well-researched and well-focused study concludes with an essay on sexual ideologies and ethics in the age of antibiotics. The author carefully develops a number of issues related not only to the specific problem of venereal diseases but also to the larger concern of American culture and society.

Brandt's interpretation is sound when he focuses on medical history and the general relationship of venereal disease to culture and society, but, by frequently advancing, and then qualifying, an interpretation, he exhibits the caution usually associated with a first book. If he has any pronounced leaning, it is toward emphasizing personal liberty for sexual expression in the age of antibiotics. In a concluding policy statement, Brandt criticizes existing institutionalized practices and advocates some new ones. He believes that advertising, chemical prophylactics, and other forms of preventive medicine should be widely used and that screening for venereal diseases should concentrate on the populations most likely to be infected. Although Brandt is not advancing a new idea in stating that venereal diseases should no longer be equated with sin, he does advance the relatively new idea that efforts to modify the behavior of Americans in association with combating venereal diseases should be abandoned.

What this study gains through a longitudinal study of modern venereal disease policy, it loses in not integrating the medical aspects of the campaign against venereal diseases into the larger social dynamics that contributed to the formation of an "American plan" for the management of venereal diseases. The study is especially thin in its analysis of the late nineteenth-century and early twentieth-century dynamics between the woman's, the purity, and the hygiene movements. Brandt, for example, refers to women's protest against the New York Page Law in 1910, a heated and extensively debated episode, but cites the opinions of only two male physicians and fails to discuss the charge of neoregulationism central to the critique of the law governing the Women's Court. For feminists and their allies, the Page Law and the rules of the San Francisco Municipal Clinic provided clear examples of neoregulationism overtly advocated and practiced in 1910. Furthermore, women's critiques of the venereal disease policies during the First World War are briefly summarized but given limited exposition. Had the policies advocated by Katharine Bushnell and Alice Stone Blackwell been adopted, they could have reduced VD rates in the United States. Their proposals were modeled on those adopted in Great Britain, where the opening of free clinics with attendance on a voluntary basis was credited with halving the VD rate in the 1920s.

This book deserves much attention. It is a major contribution to both medical and social history. The author writes intelligently and well about an important subject. One hopes that the book will encourage public discussion and further research into the complex, but little-researched, social hygiene movement as well as into the general subject of sex in history.

DAVID J. PIVAR
California State University, Fullerton

WILLIAM LLOYD FOX. *Dandy of Johns Hopkins*. Baltimore: Williams and Wilkins. 1984. Pp. xii, 293.

Walter Edward Dandy (1886–1946) was a pioneering neurosurgeon at Johns Hopkins who greatly increased the understanding and treatment of previously incurable diseases of the nervous system.

Born in Sedalia, Missouri, Dandy was the only child of recent immigrants from England and Northern Ireland. His father was a rare hybrid of both socialism and fundamentalism, two causes that the younger Dandy did not follow. Dandy began his medical training at the University of Missouri and then transferred to Johns Hopkins in 1907. He did particularly well with Franklin Mall in anatomy and Harvey Cushing in surgery. After graduating in the top quarter of his class, Dandy studied under Cushing at the Hunterian Laboratory of Experimental Medicine where Dandy began his long publishing career on the nervous system. The next year, he joined the surgical house staff at Hopkins, again under Cushing, but the relationship between these two men, who were destined to become leaders in American neurosurgery, soured. Cushing refused to take Dandy to Harvard in 1912, and the acrimony remained until both men died.

Dandy stayed at Hopkins, first as resident under William Stewart Halsted and later as a member of the faculty. William Lloyd Fox points to a number of Dandy's accomplishments. With Kenneth Blackfan, Dandy made an important contribution to understanding how hydrocephalus developed in children. He greatly improved means of diagnosing diseases of the brain by developing two useful ways of x-raying the nervous system: ventriculography and pneumoencephalography. Of note is that Dandy tested his new method on living children rather than on animals. He developed several techniques of removing brain tumors previously thought inaccessible to the surgeon. He also pioneered in postoperative neurosurgical intensive care.

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Fox has combed many of Dandy's publications and manuscripts. In addition, he extensively interviewed Dandy's associates. From them we get a picture of surgical training at Hopkins that differs from other accounts. Fox interprets Cushing's quarrel with Dandy as based on Cushing's inability to encourage and "bring along" students. Yet Dandy, the victim, also had periods when he unnecessarily harassed or ignored his own students—for example, Warfield M. Firor and Barnes Woodhall—and Dandy discouraged the formation of a neurological institute at Hopkins in which he would have to share power.

In his preface Fox states that he aimed his book at historical and medical audiences. Surgeons may miss a picture of a "day in the life" of a surgeon a half-century ago, and historians will note the lack of a sense of development in procedures and intellectual understanding of diseases of the nervous system. Nevertheless, the book will interest a broad audience.

PETER C. ENGLISH
Duke University Medical Center

KENNETH M. LUDMERER. *Learning to Heal: The Development of American Medical Education*. New York: Basic. 1985. Pp. xv, 346. $21.95.

This book is a brilliant reinterpretation of the origins of modern medical education in the United States. It destroys the familiar myth of a reinvigorated American Medical Association (AMA) driving the weaker, commercial medical schools from the field after 1904 and imposing new, high standards to protect its own selfish interests. It establishes irrefutably that the principal reforms in medical education came before, not after, the famous report by Abraham Flexner in 1910. It restores the changes in scientific outlook in Europe to a central role in the social transformation of American medicine. And it faces squarely the criticisms of modern writers that the "biomedical model" of practice that emerged after 1910 brought few cures, emphasized a mechanical approach to patients, and principally served the interests of a dominant industrial capitalism.

The modern medical school, Kenneth M. Ludmerer makes clear, had its origins in a conceptual revolution about how doctors should be taught that occurred in the last quarter of the nineteenth century, a time of great scientific and institutional change. The American pioneers who worked in the laboratories and hospitals of Germany after 1870 brought back an abiding conviction that real learning took place only when students were active participants in the learning process. A generation before Dewey, academic leaders in medicine preached the importance of learning by doing, critical thinking, problem solving, and lifelong education. The simultaneous rise of the modern university provided a secure new home for academic medicine, the spread of public education made higher standards in medicine feasible, and the great foundations built on industrial wealth paid for the revolution. The conceptual revolution had thus already occurred and the institutional framework was already in place before the AMA or Flexner played any role. Indeed, Ludmerer demonstrates that the average practitioner represented by the AMA in 1900 was either skeptical or hostile to the rise of academic medicine.

It is amazing how early and how deeply the present pattern of American medical education was set. By 1910, when Flexner wrote his landmark study, the outlines of the system were firmly in place and, following the consolidation of the next decade, have changed little since. The crucial role was played by academic scientists—William Welch, Franklin Mall, Henry Bowditch, Simon Flexner, and scores of others—who domesticated German science and became, with the help of university presidents and others, the premier institution builders of modern medicine. As the author concludes, academic medicine "did not emerge from the modern system of American medical education; it was the force that created the system" (p. 6).

Ludmerer sustains his remarkable conclusions by a prodigious job of research. He is the first to explore systematically the records of dozens of medical schools, some of them now extinct, and to test the older interpretations of how medical schools grew and changed. He has also examined thoroughly the records of foundations and hospitals and used intelligently a wide range of secondary works. His mastery of the historical sources is the more remarkable in light of his training as a medical internist. Fortunately, an enlightened administration at Washington University has enabled him to pursue medical history as his "clinical science." It is a stunning achievement.

THOMAS N. BONNER
Wayne State University

REGINA MARKELL MORANTZ-SANCHEZ. *Sympathy and Science: Women Physicians in American Medicine*. New York: Oxford University Press. 1985. Pp. xii, 464. $24.95.

This book is a detailed study of American women physicians based on a voluminous amount of material on such topics as sex discrimination, barriers in medical education, gender differences in medical treatment, and the lives of individual women doctors. In general, the book reaffirms and fleshes out

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previous scholarship. The strongest section of the book consists of two chapters that present a collective portrait of women doctors in the late nineteenth century. In these chapters Regina Markell Morantz-Sanchez adds to our understanding of the courage and determination of these women in the face of a sexist society. Unfortunately, she omits any discussion of race. Readers interested in a comprehensive and well-written treatment of this topic should consult Gloria Moldow's *Black and White Women Doctors in Gilded Age Washington* (1986).

Morantz-Sanchez sometimes promises more than she delivers. In "Separate but Equal," a chapter dealing with medical education, she argues that a few of the female medical colleges provided "an adequate or superior" training program (p. 72). For example, the New York Infirmary offered the first course in preventive medicine in 1868, and by 1874 a three-year medical school program became obligatory. Unfortunately, Morantz-Sanchez's assertion of separate but equal applies to only four schools. She agrees that the New England Female Medical College was extremely inadequate, but she has little to say about the twelve other women's medical colleges. Moreover, a great deal of her evidence is drawn from catalogues, annual reports, and statements by male physicians, some of whom, the author acknowledges, had an ax to grind because they wanted to keep women out of the coeducational schools. Morantz-Sanchez's conclusion is somewhat simplistic, since any real comparison would require a more systematic examination of a number of factors. As she admits, a number of the women went on to obtain a "second degree" at a coeducational school after completing a degree at a woman's medical college, which suggests they recognized that something was missing from their education (p. 395).

The book is occasionally marred when the author creates straw arguments out of incomplete quotations from sources. For example, in her discussion of the New England Female Medical College, Morantz-Sanchez claims that I have suggested that the cause of failure of the school "lay primarily with 'the resistance of the medical world to women'" (p. 82). She then goes on to argue that much of the problem lay with the school's founder, Samuel Gregory, whom she claims had a "difficult personality." But, in fact, I very clearly stated that the college's failure "stemmed from a number of problems, some of which were rooted in society and others that were of Gregory's own making" ("*Doctors Wanted: No Women Need Apply*" [p. 71]).

Morantz-Sanchez has made a contribution to the history of medical women, and I hope that future historians will continue to build on the books that have been published in the past decade. We particularly need to fill the gap between 1920 and the present. As a result of the revolutionary changes brought about by the women's movement, female medical students now make up one-third to one-half of the annual enrollment in medical schools, and we need far more historical research on this phenomenon.

MARY ROTH WALSH
University of Lowell

A. MICHAL MCMAHON. *The Making of a Profession: A Century of Electrical Engineering in America.* New York: The Institute of Electrical and Electronics Engineers Press. 1984. Pp. xv, 304. $39.95.

The year 1884 saw the founding of the American Institute of Electrical Engineers (AIEE), which in 1963 merged with the Institute of Radio Engineers (IRE) to become the Institute of Electrical and Electronics Engineers (IEEE). A. Michal McMahon's history of the IEEE was written with the sponsorship of that organization's centennial task force but with a guarantee of scholarly independence. To the author's credit, his work lacks the tone of uncritical celebration sometimes found in centennial productions. He offers us a serious and fascinating study of institutional development, comparable to Bruce Sinclair's history of the American Society of Mechanical Engineers.

McMahon's work has two main objectives: to isolate the values that characterized the electrical engineering profession and to identify the technological and industrial developments that influenced its history. He organizes his story around a set of engineers whom he takes to have been representative and a series of pivotal events that affected how engineers defined their professional roles. The individuals chosen usually turn out to be past presidents of the two institutes that joined to form the IEEE. The pivotal events are either major technological developments, such as the rise of the electric light and power industry and later the appearance of radio broadcasting, or major shifts in the relationship between the electrical engineering profession and its governmental and corporate employers. In institutional terms, much of the drama of the story emerges from the failure of the AIEE to contain the ambitions of the radio engineers for a larger and more autonomous role, culminating in the formation of the IRE in 1912 and, later, in the reluctant but inexorable movement of the two organizations toward merger in 1963. McMahon is at his best in describing these institutional dynamics.

To isolate the values that characterized the electrical engineering profession and its two major professional organizations, McMahon analyzes opinion and policy with respect to membership criteria, engineering education, professional ethics, and

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standardization. The material on changing ideas of engineering education seems to me very valuable. The discussion of professional ethics, in contrast, is scanty and refers almost exclusively to engineering consultants. On standardization, I would have appreciated more information on what the two institutes actually achieved; a case study or two would have been helpful. In general, a contrast emerges between the value orientation of the two organizations, with the AIEE taking as its primary reference the culture of the corporation while the IRE looked more toward the laboratory and "engineering science." But we should probably not make the differentiation too sharp.

The book shows some signs of haste, probably the result of pressure to meet a centennial deadline, and the pages are printed in a dense format that is not conducive to easy reading. Nevertheless, I heartily recommend it to all who have an interest in the history of American engineering. More generally, the material here is highly relevant for any history of the idea of professionalism in America. Electrical engineers wanted to be seen as professionals and to see themselves as such, not out of egotism but because professional status promised greater independence from the demands of corporate and government employers and greater influence on government policy. The latter goal was achieved in part but not, I think, much of the former.

HUGH G. J. AITKEN
Amherst College

NEIL H. WASSERMAN. *From Invention to Innovation: Long-Distance Telephone Transmission at the Turn of the Century.* (John Hopkins/AT&T Series in Telephone History.) Baltimore: Johns Hopkins University Press. 1985. Pp. xxiii, 160. $17.50.

In 1982 the American Telephone and Telegraph Company opened its corporate archives to outsiders. To define some of the initial questions that would be asked in light of the new sources, the company sponsored this book as one of three commencing a joint Johns Hopkins/AT&T series in telephone history. Although other volumes on such subjects as labor relations are promised, these first three books are all located somewhere near the intersection of engineering and corporate management. Neil H. Wasserman's topic is the American Bell Company's quest for the techniques of "loading" lines with magnetic inductance coils, the aim being to extend (effectively, to double) the range of telephonic transmission. He considers this quest from two linked perspectives—as a matter of intellectual creativity and as the inducement for Bell to begin integrating research and development with business strategy by means of an approach later dubbed "engineering economy."

Loading was indicative of a shift at the end of the nineteenth century away from the empirical mode that had characterized even Bell's invention of the telephone. To demonstrate this, Wasserman takes us on a traditional "shoulders of giants" tour from Oersted through Faraday to Maxwell, by which point, he says, matters had moved into "the upper reaches of the physicists' ethereal world," far from "the grasp of the electricians, the old-hand technologists of the time" (p. 22). A circumstance sure to confuse some readers is that, as Wasserman next shows, the essential task of rendering the complexities of Maxwell's electromagnetic theory useful for electrical engineering was performed by Oliver Heaviside, an eccentric English telegraph operator who mastered Maxwell's theories by himself, without the benefit of university training or, indeed, of any institutionalized introduction to the "upper reaches."

That point is worth emphasizing, for Wasserman casts his study of this innovation within a formalized scheme whose keystone is the "scientific base." Historians of technology have put much collective effort into discerning flows of information between communities of scientists and technologists, and many are convinced that "science" often (perhaps usually) follows rather than leads. A man who appears to have been in the classic "electrician" mold was as crucial to the loading technique as a great theorist, and one may wonder why Wasserman puts so much stock in the import of those new "Ph.D. physicists" who populate his pages.

Beyond that remains the question of how readily the model may be "generalized to other instances of technological change" (p. 119). Nevertheless, in tracing loading from its theoretical inception to an innovation consonant with the aims of management, Wasserman has done admirably. In broad contour the story has already been told, by James E. Brittain in a prize-winning article published in *Technology and Culture* in 1970. But the archival materials that were unavailable then have enabled Wasserman to present one of the most detailed elaborations of the innovative process (a two-phase process as he sees it, the first being invention and the second being implementation) that I have seen. As such, it stands with the work of Robert Friedel and those few other scholars who are investigating this essentially virgin realm.

Yet in some respects this is a remarkably old-fashioned book. Wasserman takes a stance, for example, on behalf of the general idea of abstract science as a "necessary condition" for technological innovation, a stance only slightly leavened by current critical precepts. And he displays an innocence of that useful critical perspective on the interrelatedness of

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science, engineering, and corporate capitalism at the turn of the century, a perspective whose classic formulation remains David Noble's *America by Design* (1977). Noble's uses of history can be faulted, and Wasserman may certainly contend that his book is neither a call to action nor a grand synthesis but an illuminating case study. So it is. Yet his book seems to lack an edge, a sort of detached wariness, that might have made it appeal to historians other than specialists in technology and corporate structure. As this series develops, I, for one, will be eager to see if it becomes what a promotional flyer promises: "a sophisticated analysis of the company that became the world's largest corporation."

ROBERT C. POST
Smithsonian Institution

K. AUSTIN KERR. *Organized for Prohibition: A New History of the Anti-Saloon League*. New Haven: Yale University Press. 1985. Pp. xvii, 293. $25.00.

K. Austin Kerr bases this political history on the recently consolidated records of the Anti-Saloon League and other primary sources. These include pertinent, previously unused papers of Andrew J. Volstead and President Woodrow Wilson (a closet "wet").

Acknowledging the already extensive scholarship on his topic, Kerr claims to make contributions on the background, 1893 creation, heyday, and, most notably, long decline of the league. It survived until 1964, when it became the still-existing American Council on Alcohol Problems. Kerr generally concurs with Peter Odegard's *Pressure Politics: The Story of the Anti-Saloon League* (1928) but elaborates on the league's skillful use of new management and organizational techniques. While raising a fortune at a grass-roots level—particularly through fundamentalist churches—the league tightly orchestrated activities from the national headquarters in Westerville, Ohio. National headquarters paid state administrators and field organizers, thus assuring spirited efforts even in unenthusiastic states such as Wisconsin. By reorganizing to push for a national prohibition amendment in 1913, the league kept already "dry" states involved.

Kerr agrees with James Timberlake (*Prohibition and the Progressive Movement* [1963]) that "drys" were part of the Progressive movement, capitalizing on scientific, social scientific, medical, humanitarian, and efficiency studies to discredit drink. Kerr ably shows that the "liquor trust" was a prohibitionist fiction. Brewers did not cooperate with distillers in the Model License League, a reform organization that countered prohibitionist efforts. Instead, brewers calculated that prohibition would knock out hard liquor competition and abet the growing shift to beer among drinkers. Brewers advertised their product as a low alcohol nourishment for temperate tastes. Despite pleas from Adolphus Busch and Gustav Pabst, brewers continued to compete fiercely not only with distillers but also among themselves until it was too late.

Kerr claims that, because "drys" spoke of rural righteousness versus urban wickedness, "subsequent observers, some of them scholars, adopted this rhetoric [which] was simply a shorthand way of observing religious and ethnic differences within the population that had to be conquered" (p. 145). By 1914 the league's American Issue Printing Company was cranking out ten tons of propaganda a day. This included literature in fifteen languages and *The Worker*, which blasted the "liquor trust" as "greedy capitalists."

At points, readers encounter more Ohio prohibition politics than most of us care about. Generally, however, and especially in his strong introduction and conclusion, Kerr addresses national history and historiography. Not only World War I mobilization but also the income tax amendment (which undermined the argument made by "wets" that liquor provided indispensable government revenues) helped "drys" achieve national prohibition a decade ahead of even the league's expectations.

Kerr concludes that "prohibition worked" in that it cut alcohol consumption at least in half, ended the legal liquor traffic, and ended the advertising of alcohol. "Drys," the author maintains, were not pinched-face fanatics. Many were enlightened Progressives who preferred voluntary to compulsory abstinence and realized that the noble experiment would not work unless the public was educated about the hazards of alcohol.

Nowadays, when neoprohibitionists plague drinkers and smokers, this clearly—if dryly—written reassessment offers timely insights into the organization that became a model for mobilizing a mass constituency outside the political system.

THOMAS J. NOEL
University of Colorado, Denver

CARLOS SCHWANTES. *Coxey's Army: An American Odyssey*. Lincoln: University of Nebraska Press. 1985. Pp. xii, 321. $22.95.

Coxey's army was in fact many uncoordinated groups of unemployed workingmen who struggled to Washington during the spring and summer of 1894 to demand public works programs to relieve unemployment. In many ways the workers represented part of the transition between the mob activities that characterized preindustrial societies and

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the media-oriented protest politics of the twentieth century. Carlos Schwantes chronicles the march of eight of those armies. As he relates the story, these groups truly resembled an odyssey comparable to those of classical literature. His emphasis is on the story, and the story is indeed amazing. Nearly fifteen thousand men participated in the various marches. Although Jacob Coxey promoted the idea and led the first army out of Massillon, Ohio, on Easter Sunday 1894, the great bulk of those summer soldiers started from the West Coast. Schwantes minutely chronicles their often bizarre adventures. These varied from stealing trains and being pursued by Keystone Kop–like federal marshals, to slogging through rain and mud for hundreds of miles, to floating down the Missouri and Mississippi rivers on jerry-built rafts. Few ever reached their destination, and those who did were either arrested in Washington for walking on the grass of the Capitol or languished in obscurity in makeshift camps around the city until the movement evaporated from the hot August sun and lack of media attention.

By choosing a narrative approach to his study, Schwantes emphasizes the dramatic elements of the odyssey and deemphasizes the analytical and interpretive possibilities of his subject. Perhaps the real significance of Coxey's army does lie in odyssey itself, but I still think the choice of emphasis is unfortunate, particularly because Schwantes's work closely parallels the organization and content of Donald L. McMurry's 1929 study of the movement. Both books are narrative accounts, and, although each mentions specific topics of concern to modern political and economic historians, they do so in an unsystematic manner and fail to relate their analysis to broader concerns. It is unfortunate that Schwantes chose essentially to rework McMurry rather than to inquire into the more theoretically interesting implications of the march.

This criticism is basically a matter of organization and emphasis, for within his narrative framework the author does attribute the origins of the movement to the new phenomenon of mass unemployment and the fear of corporate capitalism. He does define the nature of the movement's radicalism by identifying its closest allies as Populists and Knights of Labor rather than socialists and anarchists. And he does show the ambivalence of Americans toward the new socioeconomic system by emphasizing the wide range of public attitudes toward the marchers, who were either greeted as heroes or punished as vagrants by the townsmen along their routes.

The narrative is supported by impressive research in primary sources, principally newspapers. The notes also reveal that Schwantes did not rely on many of the more theoretical recent works about the era. This lack of a broader interpretation is the major weakness of the study.

STANLEY B. PARSONS
University of Missouri, Kansas City

CLARK C. SPENCE. *The Salvation Army Farm Colonies.* Tucson: University of Arizona Press. 1985. Pp. vii, 151. $19.95.

At the turn of this century the Salvation Army created three farm colonies: at Fort Romie, California; at Fort Amity, Colorado; and at Fort Herrick, Ohio. The idea, one dear to the heart of General William Booth and promoted in the United States by his daughter U.S. Consul Emma Booth-Tucker and son-in-law Commander Frederick St. George de Lautour Booth-Tucker, was to put unemployed heads of families on unused land where they could become self-supporting. This would not only help eliminate poverty and dependency but also help re-create a healthful balance between city and countryside. With the farm colony plan, as Clark C. Spence points out, Booth joined a long line of social thinkers who linked moral and civic well-being with agriculture. And, like another long line consisting of theorists of intentional communities with whom Spence does not connect the general, Booth intended the army's colonies to serve as models. The long-term hope was that either the government of the United States or that of Great Britain would adopt the plan on a large scale. Both governments listened politely, but neither took Booth's plan seriously.

Spence attempts to make a case for the colonies' historical significance despite this lack of governmental interest and despite the fact that only a handful of families—and these not drawn from the ranks of the abject poor—ever participated in them. The plan, he notes, generated a large measure of favorable publicity for the army, thus counteracting negative stereotypes of it as a collection of tub-thumping fanatics. But, as he also admits, the real impetus behind the improving public image of the army was its highly effective relief efforts for the urban poor. Moreover, Spence documents in convincing detail that the colonies were a serious financial drain. And an additional cost of the colonies that will occur to the reader was the diversion of the energies and talents of the Booth-Tuckers and other Salvation Army leaders away from more effective programs.

This book is a meticulously researched and well-written account of a project of real insignificance, whether one assesses its meaning for the poor in the

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United States and Great Britain or for the growth of the Salvation Army.

JOHN F. MCCLYMER
Assumption College

JANET BRUCE. *The Kansas City Monarchs: Champions of Black Baseball.* Lawrence: University Press of Kansas. 1985. Pp. x, 176. $19.95.

The central problem facing the historian of black baseball is the paucity of sources. Records were kept haphazardly, and the newspaper press covered the games of the old Negro leagues in indifferent fashion. Financial data is incomplete. Nevertheless, by culling the important black newspapers, mining more than a dozen manuscript collections, interviewing twenty-five former players, and familiarizing herself with the relevant writings on sports, baseball, black history, and sociology, Janet Bruce has produced an excellent study of the Kansas City Monarchs.

Yet this glimpse into a little-known chapter of America's past is more than a history of the celebrated Monarchs. It is a survey of black baseball from its inception to its demise. Moreover, the author, a museum director, archivist, and regional historian, has expertly woven the club's history into the social and cultural fabric of Kansas City's black ghetto.

The father of the Kansas City Monarchs was a white man, J. Leslie "Wilkie" Wilkinson, a unique figure in the history of black baseball. In 1912, he organized the All-Nations team, composed of blacks, whites, Cubans, Indians, Mexicans, and Asians. When this talented club collapsed during World War I, he took the black players as a cadre and formed the Kansas City Monarchs of the new Negro National League. A man without an ounce of racism during the heyday of Jim Crow, Wilkinson (and his family) lived, slept, traveled, and relaxed with his players for more than a quarter of a century. He had their total respect. As veteran Monarch Newt Allen said: "He was a considerate man; he understood; he knew people. Your face could be black as tar; he treated everyone alike" (p. 19).

The Monarchs were consistently the best club in their eleven years in the Negro National League (1920–30). This was also true when they were in the Negro American League, which they joined in 1937. Truly, as one writer put it, they were the "Monarchs of all they surveyed." Although the Monarchs' success owed much to Wilkinson's leadership, the contributions of many outstanding players, naturally, figured prominently in the story. Inevitably, most of them were "over the hill" when the walls of separation were breached, and, thus, their names are practically unknown today. But Hilton Smith, Jose Mendez, "Bullet Joe" Rogan, Newt Allen, "Buck" O'Neil, and John Donaldson were among the Monarch stars who might have made it to the big leagues. More familiar, however, are the names of those Monarchs who did make it: Jackie Robinson, Satchel Paige, and Ernie Banks.

If there is anything to criticize in this fine book, it might be that the reader does not come away with a clear sense of the hardships and indignities visited on black baseball players in the days before integration. However that may be, for a generation the Kansas City Monarchs were one of the two or three best aggregations in black baseball, revered at home and away. For the club's success brought the Monarchs recognition well beyond their home base. Often playing two hundred games a year, in league play and out, they became a familiar sight in urban as well as rural areas across America's heartland. The saga of the Monarchs makes a compelling story; it is well told by Janet Bruce.

EUGENE C. MURDOCK
Marietta College

KLAUS SCHWABE. *Woodrow Wilson, Revolutionary Germany, and Peacemaking, 1918–1919: Missionary Diplomacy and the Realities of Power.* Translated by RITA KIMBER and ROBERT KIMBER. (Supplementary volumes to *The Papers of Woodrow Wilson.*) Chapel Hill: University of North Carolina Press. 1985. Pp. ix, 565. $36.00.

Here is a detailed study of a year and a half of American diplomacy toward Germany at the end of World War I—from January 1918 and the Fourteen Points speech of President Woodrow Wilson down to June 1919 and the signing of the Treaty of Versailles—in which Klaus Schwabe sets out what by any measure was a tragedy: the failure of American liberals to support German democracy. At the outset, people of good will in both countries seemed to be in agreement. The Fourteen Points were enormously attractive. The German government eventually appealed to Wilson for an armistice on the basis of the points. But as months passed the situation changed. The author believes the reason was a combination of conservative advice by members of the inquiry (notably, Charles H. Haskins, Charles Seymour, and Wallace Notestein), together with pressure from Entente leaders in Paris and from Republican and other conservatives at home. The resultant Treaty of Versailles, a punitive peace, confused liberals in the United States, divided them in Germany, and led to disaster.

In this translation, abridgment, and updating of a book published in 1971 the author uses the best American and German soruces, not merely pub-

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lished books and articles but also manuscript collections and archives. The volume is a triumph of what the late Samuel Flagg Bemis liked to describe as multiarchival research. It shows how far advanced, indeed how international, scholarship on the war of 1914–18 has become. This new international scholarship is frank and open; it is not committee scholarship hesitant to draw conclusions. Schwabe brings his material together in a remarkably wide-ranging final chapter. He believes that the American president showed little sensitivity to the domestic political prestige of the Weimar Republic; Wilson simply gave in to Entente leaders in determination of Germany's eastern borders, in the war-guilt article, in inclusion of pensions within the reparations figure, and in refusal to allow Germany immediately to enter the League of Nations. The president had plenty of information about Germany but tended to compromise—a curious fact, considering his reputation for inability to compromise. To be sure, if he had opposed Entente desires, particularly those of France, his country would have had to be a guarantor of whatever happened, and American domestic politics would not have allowed that. Another of the author's interesting observations, on a well-known moot point, is that containment of bolshevism "was of top priority . . . only in a strictly defensive sense. Neither Wilson nor his advisers expressed the slightest interest in Germany's idea of a collaboration between the vanquished and the victors in an economic, let alone in a military, offensive against Russia" (p. 397).

A marvelous accounting, this book is a great credit to its author and to the editor of *The Papers of Woodrow Wilson*, Arthur S. Link, in whose supplementary series it appears as volume 11.

ROBERT H. FERRELL
Indiana University

JOHN F. PIPER, JR. *The American Churches in World War I*. Athens: Ohio University Press. 1985. Pp. x, 230. $27.50.

This work examines mainstream Christian responses to the U.S. entry into the First World War. John F. Piper, Jr., concentrates on the activities of two key national religious groups. The General War-Time Commission of the Federal Council of Churches, an ecumenical body, looked after the interests of a number of Protestant churches. The National Catholic War Council, which in 1919 was transformed into the National Catholic Welfare Conference (NCWC), although not ecumenical in purpose, occasionally worked with Protestants and, more significantly, attempted to unite bishops behind a single, coherent social policy.

Predictably, both of the groups cooperated with the nation's war effort. Their rare conflicts with the War Department revolved around how best to serve the religious needs of a people during wartime without raising disturbing ethical questions concerning U.S. involvement in European hostilities. These church groups naturally devoted considerable attention to formulating procedures for selecting and equipping chaplains. Yet the churches seemed to have used the war as a justification for involvement in matters other than the sacramental lives of the soldiers. The churches attempted to meet the new needs resulting from wartime pressures and studied the impact that military camps had on the American communities in which they were located. Furthermore, Protestants and Catholics involved themselves in ministering to the social needs of soldiers and citizens. For the ecumenical federal council, migration of blacks to work in industry in the North as well as military racism lent a sense of urgency to religious activities on behalf of this minority. Both the federal council and NCWC devised extensive postwar reconstruction programs. In fact, the Catholic program, largely written by John A. Ryan, received endorsement from the American hierarchy as the Bishops' Program.

Piper's study is based on a commendable variety of archival sources and secondary works, but the bibliography is hardly exhaustive. The book's greatest strength—its comparative approach to Protestantism and Catholicism during the brief war period—is also its greatest weakness. By limiting his focus to the war years, Piper loses an opportunity to explore the relationship between Progressivism and the churches' responses to war and social change. To what extent were the wartime programs simply an extension of the social gospel? Furthermore, the narrow period covered by the work prevents the reader from fitting the war groups into the larger evolution of church organization. For example, American Catholic bishops during the nineteenth century met three times to consider questions of national consequence at their plenary councils in Baltimore. How did the NCWC advance the immigrant church's attempts to prioritize and systematize? Finally, the title of the work is misleading, for Piper does not discuss the peace churches, their organizational development, and their spiritual and social ministries during America's brief involvement in the war effort. By including an analysis of the peace churches, this work might have directly faced some of the ethical questions about the collaboration of the churches with the war effort. Nonetheless, Piper's book is a good introduction to a topic that has been neglected in recent scholarship. He ably summarizes the church groups' positions on social and spiritual questions and correctly observes that, like government, the churches tended to centralize

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power and coordinate programs in the interest of winning the war.

ANNE KLEJMENT
College of St.Thomas

THOMAS G. ALEXANDER and JAMES B. ALLEN. *Mormons and Gentiles: A History of Salt Lake City.* (Western Urban History Series, number 5.) Boulder, Colo.: Pruett. Pp. 360.

This book is a solid achievement. It is a useful addition to the successful Western Urban History Series and to the growing body of literature on the history of western American cities. The book has several appealing qualities, not the least of which is a long chronological framework that carries the history of the city from its origins as a Mormon utopian experiment to its present-day urban maturity. In addition, the book provides a welcome relief from the usual stress on the economic origins of cities and instead highlights the constant interplay between secular and religious influences in the city's early history. Finally, the book is written in a narrative, chronological style that is punctuated by a periodization unique to Salt Lake City rather than by one derived from standard national history. Although Thomas G. Alexander and James B. Allen treat such topics as industrialization, Progressivism, the New Deal, and war, they do not lose sight of the local variations in the history of Salt Lake City. Still, it would seem that the history of this Mormon capital has moved steadily toward the mainstream of American urban development, especially in the twentieth century. Despite having been conditioned, in a major way, by religious influences for perhaps longer than any other American urban area, Salt Lake City in 1984 very much resembles other American cities.

Perhaps the leading theme of the book is paradox. Salt Lake City grew up as a Mormon refuge from the persecutions of the outside world only to succeed, in large part, because of the economic influence of railroads, mining, migration to the West, and other forces thrust on the city from the outside. Moreover, despite conflicts with the government, the city nonetheless became heavily dependent on the modern nation-state, especially on New Deal and defense spending. The authors succeed especially well in developing these contradictions. By contrast, the book's shortcomings are distinctly minor. The first part of the book concentrates on Mormons and Gentiles somewhat more than on the process of urbanization itself; the chapter on the Second World War does not fully exploit that subject; and at some points this book about western urban history is not linked closely enough to the growing body of literature on American urban history. Still, this work is largely successful and fills an important gap in the historical literature on American cities.

ROGER W. LOTCHIN
University of North Carolina

CANDACE FALK. *Love, Anarchy, and Emma Goldman.* New York: Holt, Rinehart and Winston. 1984. Pp. xix, 603. $25.00.

ALICE WEXLER. *Emma Goldman: An Intimate Life.* New York: Pantheon. 1984. Pp. xix, 339. $19.95.

Lived in its entirety on a dizzying precipice, the life of this feared, despised, and, hence, fascinating American radical has regularly compelled biographers to try to capture it in full intensity. A serious student, however, must consult each study because each spotlights a separate aspect of Emma Goldman. Richard Drinnon's *Rebel in Paradise* (1961) has been a standard reference for Goldman's crisis-ridden years, supplemented by Goldman's own *Living My Life* (1931). The recent appearance of two new scholarly biographies will likely not change that, because Drinnon's study remains the most satisfying narrative account of this civil liberties heroine and her repeated conflicts with authorities wherever they tried to confine her innovative spirit within boundaries—those of family, workplace, and, principally, the political arena.

Drinnon provided the chronological span of Goldman's life from birth to death, whereas Alice Wexler's study, thorough in other respects, ends with Goldman's deportation to the Soviet Union in 1921 with friend and lover Alexander Berkman. Wexler, therefore, omits the important years under the early Bolshevik regime, her disillusionment with the communist state and the onset of profound pessimism, her long exile in France and Canada highlighted by the brief term during the Spanish Civil War as propagandist to the English-speaking world for the Spanish Anarchist Federation, additional love affairs, and death in 1940 at age 71. But Candace Falk, while taking the reader through all the years of Goldman's life, gives a disproportionate amount of space (nine chapters of eighteen) to a romance that lasted a mere decade.

This is not to say the new biographies are not well researched and wholly credible. In fact they are, and they illuminate previously unexplored facets of the flamboyant anarchist's personality and experience, because both authors consulted a newly found correspondence between Goldman and renegade physician Benjamin L. Reitman, who was, after Berkman, the most significant of her lovers. Falk uncovered this exchange in 1975 in the storeroom of a Chicago guitar shop. The wild eroticism in the letters underscores what Goldman scholars knew all

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along: the unbridled passion of her personal life paralleled the political passion she unleashed in her labors for anarchism. Despite the common fervor in her private and public self, the letters reveal very human contradictions between her philosophical ideal and her behavior.

As an original thinker who amalgamated the anarchist theory espoused by Johann Most with an individualist version of anarchism coming directly from the American intellectual heritage, Goldman made "free love" intrinsic to it, using the term to mean love untrammeled by legal formalities and social convention, uncorrupted by possessiveness. When Reitman exercised the privilege with a parade of episodic *amours*, however, Goldman could not contain her feverish jealousy, soundly upbraiding him and signing her letters with a loving but authoritative "mommy." The signature was clearly a challenge to the influence and claim Reitman's actual mother exercised over him, a torment on yet another level to Emma, ten years Reitman's senior. Much later, exiled, residing in France with the aging Berkman and his female "companion" in an uncomfortable *ménage à trois* in the cottage given her by Peggy Guggenheim, try as Goldman might she failed to conceal her envy and bitterness toward the young rival. Moreover, the letters show that self-doubt plagued this woman whose public image radiated sublime confidence.

Throwing a feminist perspective on this life, Falk and Wexler portray a woman caught in the ambiguity that has beset nearly every woman who acts in the public sphere. Because the men in her life wanted to domesticate her, she was forced to leave them. Unable to bear children without major surgery, she declined it and proposed to mother the world. Falk shows that Goldman's much younger lovers saw her as maternal symbol, to her dismay, when she was in her fifties and sixties. Indeed, some who encountered her in private social gatherings found, with Elizabeth Gurley Flynn, that Goldman was a "mild-mannered, motherly sort of woman" (Wexler, p. 168), a surprising discovery given her electrifying lecture platform presence and reputation as dangerous pariah. For the cause of anarchism she yielded the comforts of home, showing both great strength of will in making the choice and pluck in sticking to her course. Falk shows us a personality strong in endurance (arrested repeatedly, she was many times imprisoned); in loyalty and forgiveness (for his infidelities, she forgave Reitman; for ignoring her own role in the birth control movement, she forgave Margaret Sanger); and in rationality, which made her the darling of the Greenwich Village and Los Gatos intellectuals. The rationality foundered only on her blind, unreasoning passion for Ben Reitman.

Wexler analyzes Goldman's actions and devotes one chapter, superbly entitled "The Sublime Madness of Sex," to the Reitman affair. Falk employs extensive excerpts from the correspondence, reserving interpretive commentary for a final chapter. The two biographies rebuild in human proportions the courageous civil liberties martyr that Drinnon erected twenty-four years ago. Goldman's autobiography does this to a limited degree, but she was not candid about her past emotional turmoil. Nor did she reveal to her reader the intensity of the relationship. Falk and Wexler illuminate this figure, whose stature is not diminished a whit by the fresh revelations.

Although the older study best chronicled Goldman's encounters with police, local courts, J. Edgar Hoover, and the Department of Justice and explored most thoroughly the popular and press reactions to her activities, Wexler's book provides the clearest review of intellectual currents absorbed by Goldman, beginning with her Russian childhood and the extraordinary impact of one female instructor who introduced her to the ideas of the European Enlightenment, continuing into her introduction to Nikolai Chernyshevskii (whose heroine Vera Pavlovna in *What Is To Be Done?* made a powerful impression on the young Emma), Pierre-Joseph Proudhon, Michael Bakunin, Peter Kropotkin (who became her special hero), and others in the anarchist pantheon. Wexler chose her title well. Her study is of Goldman's mind and character in action and also in reaction to society's reception of her presence. It is "an intimate life" without a primary focus on sexual relationships; one's ideas are, after all, part of one's inmost self. But the author laces the story with the threads of contemporary history as well, so the reader is never lost in a subjective swirl.

Falk's longer study emphasizes Goldman's sexual life. Indeed, love figures in it more prominently than anarchy, and Falk has given the reader a clearer perception of the fallibility and basic human qualities of her subject than other Goldman biographers have done. A scholar seeking to understand this multidimensional celebrity now has more reading to do. Both new studies must be consulted, along with the older ones.

CONSTANCE ASHTON MYERS
North Augusta, South Carolina

KENNETH T. JACKSON. *Crabgrass Frontier: The Suburbanization of the United States*. New York: Oxford University Press. 1985. Pp. x, 396. $21.95.

This is the most important book on the history of American suburbs to appear since the publication of Sam Bass Warner's *Streetcar Suburbs* in 1962. Kenneth T. Jackson, one of this country's leading

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urban historians, has written the first comprehensive history of suburbanization in the United States, a superb analysis of the spatial, political, and economic dynamics of metropolitan deconcentration from the early nineteenth century to the present. Jackson asked why cities in the United States (compared to those in Europe) decentralized so quickly and with so little apparent opposition; he concludes that "there were two necessary conditions for American residential deconcentration—the suburban ideal and population growth—and two fundamental causes—racial prejudice and cheap housing" (p. 287). Drawing on an impressive array of familiar as well as little-used sources, Jackson makes effective use of regional comparisons that illuminate the national impact of suburbanization and international comparisons that demonstrate the importance of locating suburbanization within specific historical contexts.

This book is rich in material and insights, ranging from an exploration of Brooklyn as the first commuter suburb to the controversial prediction that the United States will remain the only suburban nation. One of its most important contributions is Jackson's masterful analysis of the role of the federal government in promoting suburbanization. Every federal policy maker ought to read chapter 11, which examines the ways in which federal housing subsidies profoundly altered the nature of the American housing market. The first historian to make systematic use of the Residential Security Maps of the Home Owners Loan Corporation (HOLC), created in 1933, Jackson traces the effects of the HOLC appraisal system, which "undervalued neighborhoods that were dense, mixed, or aging" (p. 197). The HOLC continued to make loans to homeowners in the devalued areas, but, when the Federal Housing Administration (FHA) acquired the HOLC maps and rating system, the use of "redlining" became widespread. The bureaucrats who made decisions about neighborhood quality based on race and ethnicity advanced—perhaps unwittingly—patterns of downtown decline and inner-city deterioration. Although "the same result might have been achieved in the absence of all federal intervention" (p. 218), Jackson's analysis emphasizes the important differences between the behavior of the HOLC, which created the maps but ignored its own classifications when giving out loans, and the FHA, which openly supported racial segregation and favored white, middle-class, homogeneous suburbs. In the final analysis, "the simple fact is that the various government policies toward housing have had substantially the same result from Los Angeles to Boston. The poor in America have not shared in the post-war real estate boom, in most of the major highway improvements, in property and income-tax write-offs, and in mortgage insurance programs" (p. 218).

This is a landmark publication, a book of major scholarly and practical importance. The political, economic, geographical, and structural underpinnings of the suburbs emerge with force and clarity. Jackson has given other suburban historians both a standard of scholarship to emulate and a valuable resource.

MARGARET MARSH
Stockton State College

ROLAND MARCHAND. *Advertising the American Dream: Making Way for Modernity, 1920–1940*. Berkeley and Los Angeles: University of California Press. 1985. Pp. xxii, 448. $27.50.

This gracefully written and beautifully illustrated book skillfully explores the complex relationships between advertising and American life in the interwar years. In many ways, Roland Marchand argues, advertising represented modernity: it celebrated the new, emphasized the stylish, and enhanced the economy's efficiency and dynamism. Nonetheless, people in advertising agencies, influenced by their own class situation and cultural values as well as by what they understood consumers wanted, believed that Americans yearned not only for modernity but also for a personalized way of life, one that stood in opposition to many elements of the new consumer culture. Consequently, advertisements simultaneously promoted modernity and tried to mediate its impact.

Neither agencies nor consumers, Marchand argues, expected advertisements to reflect reality; instead, they understood that the media envisioned dream worlds to which buyers aspired. A tension nonetheless remained between what advertisers promoted—obsolescence, modernization, and extravagance—and the refinement they believed in. Advertisements offered an American dream built on new commodities at the same time that they stressed personal relationships, human scale, and the traditional values of individualism and community. Cumulatively, advertisements helped Americans adjust to a world of goods, soothing the pains of modernity. Advertisements thus served therapeutic and educational as well as economic functions.

The book rests on an immense amount of research and is informed by insights drawn from the work of anthropologists, folklorists, sociologists, and social and cultural historians, literary and cultural critics, and psychologists of perception. Marchand skillfully locates advertising executives and copywriters in their social and professional situation, enabling him to understand the tensions in their

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rhetoric and their distaste for what they saw as the vulgarity and fickleness of members of their audience, especially women. Exploring the manner in which body language, color, tone, style, symbolism, dress, photography, and art convey meaning, Marchand approaches advertisements as cultural texts, compellingly analyzing them as social tableaux, parables, and visual clichés.

The book's very considerable strengths outweigh the limitations of its scope. Despite the subtitle and an important chapter on the 1930s, there is little coverage of the period after 1936. Moreover, the book would have benefited from a fuller exploration of the relationship between advertising, politics, and corporations. Marchand might have gone farther in his effort to determine the extent to which advertising reflected the wishes and fantasies of consumers.

Nonetheless, this is the most important and original book yet written on the history of American advertising. It contributes to the history of women, family, mass media, and business. Carefully measuring advertisements against social and economic data, Marchand looks at ways in which advertising did and did not reflect realities of American life. He examines the role of advertising in the dissemination of American ideology and culture. He explores the way advertising negotiated between often-contradictory aspects of modernity. He convincingly analyzes the relationship between advertising and other forms of mass media. Finally, Marchand provides a model for cultural analysis of visual documents in a historical setting.

DANIEL HOROWITZ
Scripps College

GEORGE GURNEY. *Sculpture and the Federal Triangle.* Washington, D.C.: Smithsonian Institution Press. 1985. Pp. 464. $39.95.

Between 1926 and 1938 the federal government built seven massive neoclassical buildings, including the Department of Commerce, the Department of Justice, and the National Archives, in that area of Washington, D.C., known as the Federal Triangle. As George Gurney shows in his exhaustive book, the architects appointed to design the new buildings were committed to the Beaux-Arts principle that sculptural decoration was "a necessary ingredient in the image of the building" (p. 13). As a result, forty-four sculptors produced 112 works in what was the largest and last embodiment of the Beaux-Arts tradition in America.

Gurney uses a wealth of archival material as well as interviews with many of the surviving artists to provide a detailed and fascinating account of the collaborative effort that was necessary to complete the most ambitious architectural ornamentation program that the government had ever attempted. He indicates that the architects, sculptors, and federal bureaucrats worked together effectively, if not harmoniously. Between the world wars, architectural sculpture underwent a transition from Beaux-Arts academicism to a more modern idiom. Before the New Deal, the architects for public projects chose the sculptors with whom they would work, but after 1934 one of the newly initiated government art agencies, the Section of Painting and Sculpture of the Procurement Division of the Department of the Treasury, gave out the commissions for the Federal Trade Commission building and the interior of the Post Office Department. As Gurney shows, this change led to a shift from using older, academic, establishment sculptors, who received most of the earlier commissions, to using younger, more modern sculptors, who gained a greater share of commissions in the later years. As a result, the symbolic, classical figures representing abstract values predominated in most of the structures in the Federal Triangle up to 1933, but more simplified and realistic representations of New Deal values prevailed in the later 1930s.

The book's strength lies in its careful reconstruction of the inner workings of this massive public art project. Its weakness is in its neglect of the broader societal trends that influenced the program. Although government changed in ideology from the conservative, Republican administrations of the 1920s to the liberal, Democratic regime of Franklin Roosevelt, Gurney provides little analysis of the effect this had on the development and implementation of the project. He does comment on the shift away from the Beaux-Arts style to a more modern one, but one wishes that he had provided a more sustained analysis. None of this, however, is to take away from the substantial accomplishment of Gurney's work, which is of great value for both art historians and historians.

GERALD E. MARKOWITZ
John Jay College
City University of New York

JOHN H. LENIHAN. *Showdown: Confronting Modern America in the Western Film.* Reprint. Urbana: University of Illinois Press. 1985. Pp. x, 214. $10.95.

Any regular observer of developments in academic disciplines over the last decade would note that scholars have become increasingly interested in the meaning of popular culture in various societies. In the history profession, this trend represents a growing sensitivity to the importance of widely diffused games, rituals, and artifacts as new sources for answering major questions. In the field of American

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history until quite recently, research along these lines has been concentrated in periods before 1945. Yet, with a former movie star in the White House, even the most skeptical would have to recognize that the interaction of popular culture and larger sociopolitical developments of the twentieth century is a subject that begs for comprehension. The question of how one explores the issue, however, is as wide open as the various methods in the discipline itself.

John H. Lenihan's book demonstrates how an approach identified with American studies can inform our understanding of postwar America and film history. Lenihan has examined all the major western films of the period after 1945, making a thematic analysis of their contents. Within that context, he shows that, as a myth and symbol, the western did not die with the end of the frontier. Rather, after World War II that genre provided a focal point for dramatizing anticommunism, group conflict, and anxiety. In one of his most insightful chapters, Lenihan shows how themes of alienation and criticism of racial intolerance were not restricted just to intellectual writing or civil rights activists. Indeed, they were dominant themes of westerns in the 1950s.

Through this and many other insights, Lenihan has advanced our knowledge. Yet future research in the field needs to overcome the limitations of this book. Historians studying popular culture in early modern Europe, such as Natalie Zemon Davis and Carlo Ginsberg, have demonstrated that we cannot assume that, simply because texts were popular, they reflected the values of the people. In this vein, Lenihan has not shown us who the consumers of westerns were or whether they agreed with the films. Nevertheless, he asserts that the westerns mirrored larger historical themes. Yet rarely does he suggest how the film makers fitted into the larger social process and might have used the medium to dramatize their own quest for meaning and ideology. In other words, the book displays little understanding of how postwar Hollywood was filled with battles over power and ideas.

Finally, the book falls prey to a common failing of historians when they examine film. Most of us were trained to examine written sources and to overlook aesthetic forms. At best, historians view art as a mirror of themes derived from other, more "substantial" sources. Thus, Lenihan intelligently explores film plots, but, when it comes to analyzing their central component, the visual or aesthetic forms, he provides little analysis. Furthermore, since the author sees film as mirroring trends derived from other historians' current understanding of the era, he does not use the art form to alter our thinking about the postwar era.

Nonetheless, these criticisms should not detract from Lenihan's accomplishment. In this well-researched book, complete with a valuable bibliography, he convincingly documents how western films mirrored major cultural trends of the postwar era. Furthermore, he demonstrates that the mass media duplicated criticisms that other historians have dismissed as the sole preoccupation of high culture critics. That alone is a valuable contribution.

LARY MAY
University of Minnesota

J. FRED MACDONALD. *Television and the Red Menace: The Video Road to Vietnam.* New York: Praeger. 1985. Pp. xii, 277.

For twenty years, J. Fred MacDonald argues, television served not only as America's most popular mass medium but also as an ardent supporter of rigid cold war orthodoxies. The blacklist kept Stalinists and many innocent progessives off the air. The unaffected, in news and entertainment programs alike, paid homage to the American military while engaging in the most negative stereotyping of the Soviet Union and communist sympathizers. Network newscasts and documentaries uncritically reported successive presidential administrations and their foreign policies, including America's escalating involvement in Vietnam. As a result, MacDonald maintains, "years of misrepresentation on television actually led the American people toward that battle" (p. vii).

Although MacDonald can be faulted for not consulting some published and unpublished analyses of television programs in the 1950s and 1960s, some of which would have complemented his study, his reliance on original materials, both scripts and reviews of programs, is impressive. Most frequently cited are those series dealing directly with cold war themes, although even ostensibly light fare such as westerns and cartoons, MacDonald suggests, sometimes engaged in propagandizing. Reviewing the network's early handling of the Vietnam war itself, MacDonald confirms earlier studies by Todd Gitlin, George Bailey, and Daniel Hallin, who contend that most television journalists waited until late 1967 and 1968 before beginning to question American intervention.

Perhaps because of the author's own passions about Vietnam, his book both informs and exaggerates. Television did quickly emerge as the most popular of the public arts, as MacDonald notes. Yet the mindless cold war imagery that he decries could also be seen in most daily newspapers, popular magazines, and films. That television constituted the most powerful mass medium does not, however, mean that it was only one. (Put differently, without

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television, Americans might just as well have relied on *Life* to lead them into Saigon.) Nor did television's advent necessarily transform the American family room into a brainwashing chamber. Such subtleties elude MacDonald, who writes, "Whatever 'video' said affected millions every minute of every day" (p. 148). Similarly, inadequate care is taken measuring audiences; the A. C. Nielsen annual listing of the twenty-five most popular series in the 1950s did not include any of the nineteen anticommunist spy series (including the notorious *I Led Three Lives*) that, along with other programs, MacDonald offers as evidence of television's early cold warmongering. Finally, MacDonald's sense of television as "highly politicized theater" (p. 200) causes him to see anti-Red every hour, on every channel, in every genre. Although this perspective stimulates reconsiderations of some popular programs, it can be carried too far. Were *Gomer Pyle, U.S.M.C.* (one of the "militaristic situation comedies" of the 1960s [p. 200]) and *Rocky and His Friends* (a cartoon series) extensions or gentle satires of a cold war mentality?

Mostly by omission, MacDonald refuses to consider that many if not most popular television series in the 1950s and 1960s endeavored to avoid political images. Widely watched programs like *Dragnet* and *December Bride* sought to entertain and, by succeeding, sell advertisers' wares. Among the programs that were viewed least were the dreary network documentaries promoting one administration policy after another. Given a choice, most Americans did not want to spend their evenings with John Foster Dulles or Dean Rusk. And that failure in public discourse, not "propaganda" in television news and action dramas, may explain television's responsibility for the Vietnam war.

JAMES L. BAUGHMAN
University of Wisconsin

LESLIE ANDERS. *Gentle Knight: The Life and Times of Major General Edwin Forrest Harding*. Kent, Ohio: Kent State University Press. 1985. Pp. x, 384. $27.50.

For forty-one years, as Leslie Anders writes, Edwin Forrest Harding served in the United States Army. During this time he studied and taught at West Point, prepared men for battle and led them at Buna in World War II, met and developed deep friendships with leading military figures, and, perhaps most significantly, helped shape whatever within the American military system of his time might be called "intellectual." He was a decent, somewhat scholarly man who consistently cared more for those whom he led than for military glory. For this his career suffered, but in the end it is the reason he deserves our attention.

Harding knew George Marshall, Matthew Ridgway, Omar Bradley, and Robert Eichelberger well and was often close enough to observe Dwight Eisenhower, Joseph Stilwell, Douglas MacArthur, George Patton, and J. Lawton Collins. His correspondence with and evaluations of these men add some new detail and vision to our understanding. As editor of *The Infantry Journal* in the 1930s and as the first chief of military history after World War II, he clearly raised the literary and scholarly standards of army publications (despite his devotion to doggerel.) That he, Marshall, and Eisenhower were all intent on "telling it [World War II history] like it was" (p. 298) established an honorable precedent to which U.S. military historians have since consistently attempted to adhere, often in the face of pressure, from both inside and outside the army. Even with these achievements, however, Harding's career was difficult, and he will probably be remembered most as the general officer MacArthur and Eichelberger relieved at Buna after he refused to take the risks that might have achieved victory sooner but that certainly would have killed many more of his troops. Not until 1957, with the publication of the official army history of the battle, was Harding's action completely vindicated.

Because of such actions, Harding merits the meticulous research and loving labor that Anders has provided. To describe a "gentle knight" in a time and context in which the values he held seemed increasingly anachronistic and personally unprofitable is to remind us today, in a world even more violent and self-interested, that some people have placed moral courage above personal gain and lived to know that their choices were correct. Still, Anders's biography is not without problems. Most serious is the author's inability to separate the important issues of Harding's life from the insignificant. One wonders why Anders gives more emphasis to the general's interest in the Little Theater movement and stock market speculation than to his formative years and family relationships. Since the author had at least limited access to Harding's widow and interviewed Harding's surviving daughter and several other relatives at length, one can only suppose that Anders's focus on the adult career, professional friendships, and hobbies of his subject resulted from the questions he asked his material. That he was not more critical of his sources or his subject remains the book's most serious weakness.

CAROL M. PETILLO
Boston College

GERALD D. NASH. *The American West Transformed: The Impact of the Second World War*. Bloomington: Indiana University Press. 1985. Pp. x, 304. $35.00.

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Gerald D. Nash has long been recognized as the historical profession's foremost explorer of the American West in the twentieth century, and this latest work will certainly add luster to his reputation. He has produced a volume that deepens our understanding of the social and cultural developments of the World War II years and that will serve as a rich lode for doctoral students prospecting for dissertation topics.

Two of the book's features stand out. The first is the range and depth of Nash's research; the bibliography is as awesome as the many natural wonders of the region. Standing in sharp contrast to the thoroughness of the investigation is the stark simplicity of the book's title and theme: "the Second World War transformed the American West" (p. vii). In four years the American West, which had been a colonial society based on the exploitation of raw materials, developed a modern diversified economy and therefore had a head start on the rest of the nation in moving into the postindustrial era. The war sparked a vast new wave of westward migration, which placed heavy strains not only on the physical resources (housing, schools and so on) of the region's urban areas but on the social fabric as well. Blacks emerged as a visible minority in the West for the first time, and Spanish-speaking Americans (Nash's preferred term) left their traditional and isolated rural communities for the jobs and lights of the big cities. Indians, too, felt this urban pull, and, although Japanese-Americans were sent packing to desolate internment camps, they, like the Indians, came out of the war experience more closely identified with the mainstream of American society. Scientific research in the West received a big boost during the war, laying the foundations for the region's future economic development. Emigres contributed significantly to the expansion of that scientific effort, but they played an even more conspicuous role in transforming "southern California from a provincial and local cultural center to one of national and international dimensions" (p. 198).

Given the size of the book, Nash has been remarkably successful in providing detailed descriptions of the trends and events of this era. The book is perhaps too detailed in some respects, in part because of the author's propensity to load down the narrative with names, but overall Nash has done a superior job of balancing fact with analysis. We are, for example, unlikely to see a better short treatment of the zoot suit riots of 1943 than the one presented here or as concise but cogent an examination of the flowering of psychoanalysis on the West Coast during the early 1940s.

Having lavished such praise on the book, it is probably unfair to criticize Nash for not doing more. The economic changes that created the climate for the transformation described here are treated only briefly; another volume is projected on that subject. No mention whatever is made of the politics of the era or how the war may have influenced the West's relations with the federal government. But Nash is most vulnerable in regard to his theme. The Second World War certainly left its imprint on the West, but might it not have been only transitory if four years of total war had not been followed by four decades of cold war? It is a question worth further exploration by this intrepid pioneer.

MARK I. GELFAND
Boston College

BRYCE WOOD. *The Dismantling of the Good Neighbor Policy*. Austin: University of Texas Press. 1985. Pp. xiv, 290. $27.50.

A quarter-century ago, Bryce Wood published *The Making of the Good Neighbor Policy*, a tediously written but persuasively argued volume that related how, in the aftermath of the Nicaraguan and Cuban interventions of the late 1920s and early 1930s, the administration of Franklin Roosevelt shaped the Good Neighbor policy. That policy, Wood reminds us in the opening passages of this work, was "not simply rhetoric, as were the Good Partner and other so-called policies that followed it" (p. ix). Adopting the principles of nonintervention, noninterference, and reciprocity, the Roosevelt administration forged a new relationship with the other countries of the Western hemisphere that provided a foundation for the wartime alliance.

World War II strained the Good Neighbor policy, largely because of a debate within the Roosevelt administration (symbolized by the frequent clashes over Latin American policy between Cordell Hull and Sumner Welles) over how to deal with Argentina and other reluctant converts to the Allied cause. Hull, the Wilsonian, wanted to get the Latin Americans lined up behind the United States as preparation for joining the world community. Latin Americans, of course, saw things differently. The shattering impact of the world depression of the 1930s and Roosevelt's professions of kinder treatment from the United States had persuaded not a few of them, especially the nations of South America, that they could shape not only their domestic but also their foreign policies without harassment from the United States.

Welles understood their sensibilities, especially their sometimes-intense nationalism and their reluctance to be dragged into a wartime alliance. At the Rio conference of early 1942, Welles defied Hull by accepting a moderate proposal allowing the Latin American countries to delay breaking off relations with the Axis powers. Most not only broke relations

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but even declared war. The South Cone nations, for differing reasons, proved defiant. Hull launched his bureaucratic crusade to defeat Welles and, ultimately, to bring what in his mind were "proto-fascists" in Buenos Aires to heel. By war's end, Hull's approach appeared successful. Argentina declared war against the Axis and, with American blessing, joined the United Nations.

Yet the price of the victory had been high. Even as the cause of internationalism was triumphing in the United States, Latin Americans were emerging from the war determined that the United States must commit itself to regionalism. In Wood's view, the United States fulfilled the goals of the Good Neighbor policy with pledges at Chapúltepec in 1945, Rio de Janeiro in 1947, and Bogotá in 1948, which produced the Inter-American Treaty of Reciprocal Assistance and the Organization of American States (OAS). But, concurrently, older strains of the U.S. approach to Latin America were already reappearing. In the aftermath of Hull's insistence on Argentina's political conformity during the war, a number of Latin American countries, led by the Uruguayan foreign minister Eduardo Rodríguez Larreta, proposed a multilateral policy to ensure the survival of democratic regimes. The United States, sensing what this could mean for the Caribbean tinpot dictators whom it had been sustaining for years, rejected the proposal and then opted for the unilateral route by blatantly interfering in the Argentine election of 1946 with the *Blue Book* indicting Juan Perón.

Still, as Wood sees things, the Good Neighbor policy might have been saved. After all, the Rio pact and the OAS charter came later, and he credits the Truman administration with adhering to the essential principles of the policy, even though warning signals about Guatemala's leftist course were already noticeable. Eisenhower and Dulles dismantled the Good Neighbor policy, and the new administration accepted the leftist revolution in Bolivia because the revolutionaries, once in power, made sure they accommodated American wishes by offering compensation for seized properties and by purging Communists from office, thus fulfilling the old Rooseveltian notion of "reciprocity," one of the three pillars of the Good Neighbor policy. But the Arbenz administration in Guatemala proved unwilling to make such commitments, and the Dulles brothers, eager to make Guatemala a test case for Latin American loyalty in the cold war, tossed Arbenz out in the still-controversial 1954 affair.

As in his earlier book, Wood presents a detailed history of a policy largely shaped by the shifting bureaucratic tides of the Department of State. He relates an inner history of how a policy fashioned in one agency fell into the manipulating grasp of another—in the Guatemalan case, with the connivance of the secretary of state. Implicitly, Wood judges that, if only the United States had faithfully adhered to Roosevelt's original intent (Wood is reluctant to admit that the Good Neighbor policy aimed at ensuring U.S. economic primacy in Latin America), the Good Neighbor policy might have survived and the U.S. would not have returned to its old interventionist habits. Such a view presumes that Latin Americans ought to choose the standard we mark out for them and that good intentions in foreign policy produce accommodating responses in others. This is not always true, especially if the kind words of one side generally mask doubts about the capability of the other side to achieve that standard.

LESTER D. LANGLEY
University of Georgia

JAMES IRVING MATRAY. *The Reluctant Crusade: American Foreign Policy in Korea, 1941–1950.* (A Study from the Center for Korean Studies, University of Hawaii.) Honolulu: University of Hawaii Press. 1985. Pp. xii, 351.

An increasing number of scholars, including James Irving Matray, believe that the outbreak of the Korean War stimulated a crucial change in America's foreign policy. They see the conflict in Korea as the event that transformed America's containment doctrine, with its global anticommunist rhetoric, into a concrete program that was heavily reliant on military means and direct action.

Despite this realization, few studies have been made of the U.S. involvement in Korea, and this book therefore fills an important gap in the literature. More significantly, it shows how Korea came to be chosen as the test case for containment in Asia and demonstrates the resilience of the U.S. commitment to the survival of South Korea, even though many other factors, such as low strategic importance, unstable economic and social conditions in the south, and the undesirable and troublesome qualities of Syngman Rhee, dictated otherwise. In other areas, this study adds substantially to our knowledge of many issues left unresolved from the period, including a reinterpretation of Dean Acheson's famous Press Club speech of January 1950, the status and role of the United Nations Temporary Commission on Korea during the 1948 elections, and the origins of the idea of a "police action" in Korea in case of a northern assault across the thirty-eighth parallel, to name but a few.

For Matray, though, the importance of the U.S.-Korean relationship goes far beyond this, since he sees the conflict as instrumental in dragging the United States away from an isolationist past, forcing it reluctantly to adopt an internationalist stand and to embrace undesirable military means in the con-

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duct of foreign policy. Reluctance and restraint are, in fact, the organizing concepts in this study and are seen to be relevant for the whole period from 1941 to the end of the first week of the Korean War, when the Truman administration finally decided to commit combat troops to the peninsula. Matray argues that the explanatory power of these concepts can be shown, for example, in the involvement of the United Nations both in the establishment of the Republic of Korea and at the outbreak of the Korean War in 1950. More ubiquitously, reluctance and restraint are to be shown by the U.S. use of military assistance and economic aid in support of Rhee's regime rather than by a guarantee of military protection and the direct application of military power.

Although few would deny the continuing sway of the American isolationist tradition in postwar foreign policy, this legacy would benefit from being placed alongside other domestic and international factors. The reduction of military capabilities for political and bureaucratic reasons, the realization that a U.S. military guarantee might lead Rhee to launch "an invasion" north, the primacy of Europe, and the lack of familiarity with the complex political conditions in Asia—all are factors mentioned by Matray but given scant attention as possible sources of a policy of restraint. The changing perception of Soviet intentions and the hardening of domestic attitudes toward communism, leading to a greater willingness to consider military means to contain the Soviet threat, are also deemphasized as the author holds fast to his original thesis about restricted involvement.

Matray's research shows that the United States was restrained in its use of military power as the vehicle for foreign policy in Korea, but the question remains whether this is best explained by limited military capabilities or by a genuine preference for economic and political methods that were seen as more in keeping with an isolationist past.

ROSEMARY FOOT
University of Sussex

GARY HUEY. *Rebel with a Cause: P. D. East, Southern Liberalism, and the Civil Rights Movement, 1953–1971.* Wilmington, Del.: Scholarly Resources. 1985. Pp. xii, 232. $24.95.

"No one here can quite understand him," said James W. Silver of his friend P. D. East. "I think that's why he hasn't been lynched or shut up yet" (p. 94). East, outspoken editor of Petal, Mississippi, attacked racism during the civil rights movement. His irregularly published *Petal Paper* never had more than a few thousand readers and often had no subscribers or advertisers in Petal or nearby Hattiesburg, but it earned the dissenting East a reputation among liberals as well as local segregationists. Gary Huey places East in a "southern liberal reform tradition" (p. 3), but he judges East "a unique figure" (p. 212) because he lacked the liberal's usual family background, education, organizational ties, social status, and economic security. Adopted when he was five days old, he grew up in the "nomadic poverty" (p. 32) of his adoptive parents' life in sawmill camps and received little beyond an inadequate secondary education. Huey says the young East considered himself an outsider, suffered from loneliness and depression, and contemplated suicide; that pattern persisted into adulthood. After stints on a newspaper and a bus line, East worked for the Southern Railroad from 1942 to 1951; he then edited a union newspaper before starting the *Petal Paper* in 1953.

Initially unconcerned with race, East had no reaction to the *Brown* decision and even reprinted propaganda from the White Citizens' Council. By the end of 1954, he finally objected to a proposed state amendment allowing for closing the public schools, and, as a result, his paper suffered drastic losses. Spurred by his "rebellious nature and sense of decency" (p. 60), he increasingly spoke out against bigotry and violence; attempting to follow a middle path, he supported segregation. In a major change, East in early 1956 seemed to grant blacks social equality. At the same time, he ran a famous satirical advertisement featuring a jackass and inviting readers to join the "Glorious Citizens' Clans" (p. 92). East had decided to place "conscience over profit" and had "reached a point from which he could not retreat" (p. 60).

As East's attacks on racism increased, his psychological and economic problems also mounted. He struggled with an ulcer, depression, loss of friends, marital difficulties with four wives, and debts, but his biting, sarcastic commentary continued. One ad for summer-weight sheets for the KKK said, "Klanettes may enlarge the holes for arms, but your head will fit nicely through the eyelets as they are" (p. 115). His version of the Dixiecrat Bible proclaimed, "For God so loved Mississippi that He gave it 'Our Gem' [Senator James O. Eastland]" and "Jesus called them unto Him, and said, suffer the little WHITE children to come unto me" (p. 116). At the height of the Little Rock crisis, East asked, "DO YOU WANT YOUR DAUGHTER TO MARRY A FAUBUS?" (p. 119). He soon conceded that Mississippi had surpassed Arkansas in bigotry and had fallen into "Total assdom" (p. 131). At the peak of his fame in 1960, he wrote his autobiography, *The Magnolia Jungle*. Under growing pressures, East abandoned Mississippi for Alabama in 1963 and died there in obscurity in 1971 at the age of fifty.

Huey effectively chronicles East's erratic career and enlivens the narrative with generous quotations

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from East's writings. This valuable account will preserve East for all unfamiliar with the *Petal Paper*, and perhaps it will inspire needed dissenters in the magnolia jungle. East's anomalous liberalism, however, was not a well-developed ideology suited for study on its own merits. Rather, it should be studied intensively within a local context by using other local newspapers, manuscript collections, and interviews or be probed psychobiographically to determine what made East different. Huey does neither. As a result, Huey has told us a great deal about East, but understanding East has eluded Huey just as it had James Silver.

CHARLES W. EAGLES
University of Mississippi

JOHN TEBBEL and SARAH MILES WATTS. *The Press and the Presidency: From George Washington to Ronald Reagan*. New York: Oxford University Press. 1985. Pp. viii, 583. $24.95.

John Tebbel and Sarah Miles Watts have undertaken the mammoth task of chronicling the two-hundred-year history of relations between the chief executive and the Fourth Estate. It is not a happy story: the authors emphasize the adversarial nature of the relationship, detail the countless incidences of pettiness and knavery on both sides, and conclude that all presidents have disliked the press. Nor does it have a hopeful ending, considering the authors' description of the White House of Ronald Reagan, where stage direction and show business have made serious journalism nearly impossible.

As practicing journalists and professors of journalism, Tebbel and Watts are familiar with the workings of the press. They are at their best when describing reporters' reactions to the presidents and their press secretaries, and they display a clear understanding of the way in which the presentation of the news has replaced editorial policy as the most important function of the press in the twentieth century. They also have the journalist's keen eye for detail and are adept at portraying the many colorful individuals that dot the pages of their book.

But Tebbel and Watts's backgrounds also account for much of what is wrong with this study. Especially in the chapters dealing with the more recent "imperial" presidents, their analysis seems like an apologia for the press—or like a journalistic rebuttal of all those memoirs now being written by former presidential press secretaries. Tebbel and Watts are, as a result, often blind to the media's responsibility for the way things turn out, such as when they lament President Carter's difficulties during the Iranian hostage crisis that "gripped the public for so long" (p. 530), as if the press had nothing to do with them. On occasion, the authors seem unable to control their anger over presidential mistreatment of their profession. Their chapter on Lyndon Johnson, for example, reads like only the most recent salvo in the media's continuing war with that recalcitrant president. This book also exemplifies some of the worst qualities of contemporary journalism, especially its infatuation with the cult of celebrity and reporting on personalities in the style of *People* magazine. Thus, the chapter on Dwight Eisenhower focuses excessively on press coverage of his various ailments. And, if the authors really believe that John Kennedy's personal "sins" were "after all, irrelevant" (p. 483), why do they have to say so much about them in their thin chapter on his presidency?

As journalists, Tebbel and Watts do not make very good historians. Although they draw on the works of many scholars—and repeat many of their errors—they never quite manage to achieve a synthesis. Thus, contradictory statements appear on the same page, often with no attempt at an explanation. Worse, their discussion of the historiography of the various presidents and their presidencies is too much like the halting efforts of beginning graduate students, especially when they write repeatedly of "the revisionists" as if they represented some sort of a unified force in historical scholarship. For all of its faults, this book seems destined to replace James E. Pollard's *The Presidents and the Press* (1947) as the standard work in the field. But that should not discourage others from trying to do better.

ROBERT C. HILDERBRAND
University of South Dakota

DAVID S. MCLELLAN. *Cyrus Vance*. (American Secretaries of State and Their Diplomacy, number 20.) Totowa, N.J.: Rowman and Allanheld. 1985. Pp. 194.

David S. McLellan is a political scientist who probably failed calculus. As a result, he writes solid, readable historical studies of American foreign relations. His earlier biography of Dean Acheson was well received. This sketch of Cyrus Vance written for the American Secretaries of State series poses some problems. In brief, it was written too soon, rests too heavily on the memoirs of participants, and is too partisan. Even Zbig Brzezinski was right sometimes.

McLellan portrays Vance as an "establishment" professional—a man who had won his spurs managing crises during the Johnson administration, most notably, in Cyprus and Detroit. Working with Averell Harriman, he very nearly succeeded in ending the war in Vietnam in 1968. Out of his experience in government in the 1960s and his observations during the Kissinger years came the conviction that maintaining detente with the Soviet

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Union was the highest priority of U.S. policy. Like Dean Rusk, he believed that, if he could push the danger of nuclear war a little further away, he could judge his years in Washington a success. The world was filled with other problems; some were intractable, but most could be kept separate from relations with the Soviet Union. Vance believed that intense competition with the Soviets would continue, but it need not—indeed, should not—be linked to the need for strategic arms limitation. McLellan labels Vance's intellectual posture "post–cold war internationalist."

Generally, Vance had the president's support. His view and approach dominated administration policy. Eventually he was defeated by two elements in the policy process that plagued some of his predecessors and will doubtless handicap most of his successors. One was the existence of the president's national security adviser, in this instance a man with ideas and ambitions of his own who was not shy about fighting for them. The second was the president's domestic political needs, which became more pressing as the election of 1980 neared. Increasingly, the president found Brzezinski's more active and more aggressive anti-Soviet posture more attractive, especially since the polls showed it played better in Peoria—hence, the president's overreaction to the Soviet invasion of Afghanistan and the rescue attempt in Iran.

McLellan is not entirely uncritical of Vance but yields nothing to Brzezinski. Zbig probably was wrong more often than not, but surely he was right on the opening to China. Surely it would have been a mistake to have failed to respond to Deng Xiaoping's imperative needs, to seize the opportunity to gain an enormous geopolitical advantage. Few of Vance's admirers—or staff—persisted in defending his position on China. McLellan does.

This book is a useful history of the foreign relations of the Carter administration, more satisfying than the memoirs of Vance and Carter. But don't miss Brzezinski's memoir.

WARREN I. COHEN
Michigan State University

CANADA

A. I. SILVER. *The French-Canadian Idea of Confederation, 1864–1900*. Toronto: University of Toronto Press. 1982. Pp. ix, 257. Cloth $30.00, paper $12.50.

The late Donald Creighton devoted considerable energy to "proving" that Canadian confederation in 1867 was not a cultural compact between two ethnic communities. His aim was not simply the pursuit of historical truth but rather the enlistment of history to support his vitriolic opposition to any extension of the rights of Canada's French-speaking minority. In his view, what was good for the "men of 1867" was quite satisfactory for Canadians a century later. The bilingualists and the constitution revisers of the 1960s were attempting to rewrite history; what is more, he maintained, they risked stirring up dissension at a time when the American threat made Canadian unity imperative.

A. I. Silver obviously has Creighton in mind when he warns that the interpretations that prevailed in 1867 are not necessarily appropriate today. Indeed, in his conclusion he expresses the hope that all Canada will one day be a country for French-Canadians. But why does he give this warning? Undoubtedly, it is because Silver is concerned that his findings might add grist to the mill of the adversaries of bilingualism.

The central thesis of this book is that the French-Canadian idea of Canada was radically transformed in the decades after confederation. In 1867, French Quebecers generally supported confederation because, through the creation of the Province of Quebec, it would guarantee an important degree of separateness, strongly desired after the imposed togetherness of the union years during which Quebec and Ontario had formed a single entity. Quebecers had little interest for French-speaking minorities outside Quebec, although in all fairness it should be stated that these minorities were tiny, unorganized, and often only recently established.

Over the next thirty years, Canada was the scene of a series of conflicts involving the rights of Catholics and French-speakers in the provinces outside Quebec. Resulting generally in defeats for the minorities, these battles brought Quebecers to believe that confederation was really based on a cultural duality and that the rights of French-speaking Catholics outside Quebec should be recognized just as the rights of Anglo-Protestants within Quebec were. The fault lay with Canada's English-speakers, who were reneging on the contract.

This is undoubtedly a careful analysis. Some of the author's methodological choices, however, are debatable. Archival collections, for example, were little used; Silver's main source of information is newspapers, which he abundantly quotes. Can we rely on the papers to formulate "the French-Canadian idea of confederation"? Silver will, of course, reply affirmatively. But, when French-language newspapers become outraged after the execution of Louis Riel, the Manitoba Métis, in 1885, Silver writes that they "exaggerated the degree of fanaticism in English Canada" and expresses doubts that the fanatics represented Ontario opinion (p. 163). One might ask why the English-speaking press is not a reliable source of opinion whereas the French press forms the basis for Silver's

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thesis! Still, Silver's conclusions appear generally accurate, and his book is a useful and much-needed updating of certain chapters in Mason Wade's classic work, *The French Canadians* (1955).

RICHARD A. JONES
Université Laval

LATIN AMERICA

ROSS HASSIG. *Trade, Tribute, and Transportation: The Sixteenth-Century Political Economy of the Valley of Mexico.* (Civilization of the American Indian Series, number 171.) Norman: University of Oklahoma Press. 1985. Pp. xvi, 364. $22.95.

How was Aztec society changed by Spanish colonial rule? This is one of the most enduring and intriguing questions in Mexican history. Ross Hassig analyzes the transformation from the standpoint of urban development. He attempts to show how, first, the Aztec conquest of other city-states and, second, the Spanish overthrow of the Aztec empire restructured the economic relationship between Mexican cities and their hinterlands. The result is an interesting comparison of urban support systems as a function of four variables: population, types of agriculture, markets, and, especially, transportation.

Hassig notes the familiar highlights of Aztec expansion, such as the organization of the military and of tribute, but, in keeping with his central thesis, he pays most attention to the pre-Columbian transport system of human porters and the complementary five-, nine-, thirteen-, and twenty-day marketing cycles. Both facilitated the flow of goods to some degree but kept urban populations small and limited their food shed to a radius of twenty-one to twenty-eight kilometers. With the rise of Tenochtitlán, the Aztec capital, all this changed. To feed the capital the Aztecs first turned to highly productive *chinampa* agriculture, the less costly canoe transport, and tribute goods. When these measures proved insufficient, the Aztecs reorganized the valley of Mexico so that it became a single productive unit. The other city-states were required to specialize in providing foodstuffs for Tenochtitlán, while Tenochtitlán specialized in finished goods, whose raw materials came from distant areas of the empire. Urban location, as well as function, changed. Lakeshore cities, which funneled goods to the capital, grew in importance, while cities that had been centers of local trade in the valley, declined. A core-periphery macroregion then prevailed in the valley.

Under the Spaniards urban-hinterland relations were, as might be expected, modified considerably. Human porters could not meet the increased demand for carriers imposed by the new interoceanic shipping schedule, especially because disease periodically reduced the adult male Indian population. Canoe transport across the lakes was undermined by climate changes and neglect. To compensate, the Spaniards introduced the more efficient mules and wagons, built better roads, and politically integrated the region more effectively. That markets were relocated and Sunday markets prohibited caused some confusion, but the Spaniards encouraged commerce. Food shortages were not as catastrophic as they had been under the Aztecs. Relying on more distant supplies and eventually subordinating many regional centers, Mexico City became even more of a primate city than Tenochtitlán. Pre-Columbian transport continued, however, well into the seventeenth century, and the Indians themselves adopted the new practices, becoming muleteers and long-distance traders. The Spaniards did not fully exploit the potential for economic integration of the viceroyalty, primarily because transportation, though improved, was still inadequate.

This is an ambitious and stimulating book. The historian benefits from exposure to models of market systems, empires, and urban networks, taken from other disciplines, that invite him to rethink his understanding of Mexican history. Hassig's detailed accounts of the influence of calendars on market cycles and the customs of human porterage are fascinating, as is his description of the role of the lakes in the valley's economy. This is a valuable contribution to the literature on relationships between city and hinterland. The broad scope of his study has, however, led Hassig into some pitfalls. The section on the post-Columbian era relies too heavily on secondary works, a few of which are outdated, and on chroniclers, such as Fr. Thomas Gage, who are notorious exaggerators. Most of Hassig's archival sources deal with legislation rather than with colonial reality. Consequently, this section includes unsubstantiated generalizations about the self-sufficient hacienda, the effectiveness of Spanish political integration, and the decay of local trade, which strike a jarring note. Also, Hassig asks the reader to accept on faith his view that transportation is the primary determinant of urban development; in particular, he too readily dismisses the impact of the state on urban trade and tribute. The book, therefore, is uneven and, in some respects, claims too much for its central theme. Nevertheless, these drawbacks are less important than the fresh approach Hassig brings to the urban history of Mexico in the fifteenth and sixteenth centuries.

LOUISA SCHELL HOBERMAN
George Mason University

SILVIA MARINA ARROM. *The Women of Mexico City, 1790–1857.* Stanford: Stanford University Press. 1985. Pp. xii, 384. $42.50.

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Silvia Marina Arrom has written a path-breaking study, the first detailed social history of Mexican women across class and institutional lines. Meticulously analyzing a wide range of sources (notarial and court records, censuses, periodicals, legal handbooks, travel accounts) and elegantly balancing quantitative methods with other approaches, she has produced a finely crafted monograph, informed in its theoretical assumptions and rich in empirical data.

Did liberal ideas, she asks, alter gender roles in the largest metropolis in the Western Hemisphere at the beginning of the nineteenth century? Bourbon policy sought to mobilize women for work and motherhood by promoting their entry into the trades and education. Changes in attitudes toward women accelerated in the wars of independence, after which elite women were sometimes active in petitioning the government and in charitable activities. The notion of women's social usefulness gained ground; the population of cloistered nuns, for example, fell by nearly 40 percent from 1790 to 1850 while the service-oriented orders grew.

Nonetheless, the employment opportunities for women were severely constricted by postindependence economic recession and political instability. In 1838, only one-sixth of the women of Mexico City had attended primary school, the highest educational level available to them at a time when the first women's colleges were opening in the United States. Vocational training intended for poor women did not materialize, and their opportunities for gainful employment shrank. Although fully one-fourth of the city's women worked, they continued to be concentrated in domestic service and in the food and apparel industries. Middle-class women, however, encountered expanding possibilities, especially as teachers.

Although persistent disdain for women's work and a constricted job market made patriarchal protection attractive, the author's demographic analysis challenges the stereotype of marital domesticity. Mexico City women married late, at 22.7 years old; most were widowed after they turned 40; and one-sixth never married at all. Widows and single women who were well-to-do could more easily be autonomous and were therefore overrepresented among female heads of households. Spinsterhood, which bore no stigma, may have been encouraged in tight economic times by an inheritance system that forced parents to divide their belongings among all children. If the system limited women's chances for marriage, it also provided income. In contrast, Caste and Indian women, who had no independent wealth, had to marry because children were an insurance against sickness and old age; if widowed or abandoned, these women preferred living with relatives to heading their own households.

In her study of Hispanic law, postindependence legal discourse, and ecclesiastical divorce court, Arrom notes a subtle but contradictory weakening of patriarchy. Jurists viewed the husband's right to kill an adulterous wife as too extreme. Laws against domestic violence were tightened. Legal codes lowered the age of majority and released single adult children from *patria potestas*. The growing respect for women's competence and the prestige of motherhood helped grant widows guardianship over their children. Some argued that married women's authority over childrearing should be expanded and that mutual consent was the best method for managing families. The growth of individualism led to greater acceptance of divorce, proposals for divorce reform, liberalization of procedures, and a weakening of the principles of canon law that subordinated personal happiness to the institution of marriage. But, if the ideal of marriage based on love implied an incipient reassessment of sexual power relations, the weakening of the church was a double-edged sword, for secular law more clearly legitimized the double standard in its greater tolerance of male adultery. Individualism tended to benefit men, widows, children, and single women more than it did wives, whose subordination to husbands was essential to the persistent corporate view of social harmony.

Discrete gems enrich this impressive volume: the description of working conditions in the royal tobacco factory, the portrait of poor women balancing family responsibilities with earning a living, and the detailed discussion of Hispanic law. I would argue, however, with the conclusion. Having shown that conditions in the postindependence period dashed earlier options for women's emancipation and provided space for the reassertion of male dominance, Arrom suggests that, in the late nineteenth century, Catholic and corporate traditions merged with the Victorian ideal of female spirituality and domesticity in such a way as to limit women's possibilities. In fact, once economic growth and political stability were reestablished, the very statesmen who put women on Victorian pedestals opened significant educational opportunities for them, including vocational training and some professional education. Women took over the teaching profession, entered office work, and joined the growing industrial labor force. Of course, this process abounded in contradiction, and, should Arrom undertake its study, we may expect another superb monograph.

MARY KAY VAUGHAN
University of Illinois, Chicago

DUDLEY ANKERSON. *Agrarian Warlord: Saturnino Cedillo and the Mexican Revolution in San Luis Potosí.*

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DeKalb: Northern Illinois University Press. 1984. Pp. xvii, 303. $32.00.

Over the past fifteen years an astounding number of Mexicans and foreigners have ventured into the provinces to search for the social origins of the Mexican Revolution of 1910. Dudley Ankerson is among these regional historians seeking the causes of popular revolution and the subsequent emergence of new patron-client relationships between local agrarian leaders and the bourgeois state.

Ankerson has meticulously woven the political biography of Saturnino Cedillo, the famous *caudillo* of San Luis Potosí, into the events of the Mexican Revolution from 1910 to 1940. In his very fine first chapter the author contends that Cedillo and his brothers organized a grass-roots agrarian movement of *rancheros* and endebted peons embittered by the political, social, and economic domination of the landowners against the dictator Porfirio Díaz. Although the political revolutionary Francisco Madero promised to redress their agrarian grievances and to establish free municipal elections, he failed to fulfill his promises, and Cedillo rebelled again. Cedillo's movement transformed itself, over the course of the revolution, from a guerrilla movement supported by a Zapatista-like primitive peasant communism into a movement under the paternalistic care of the state. President Alvaro Obregón permitted Cedillo to keep his *agraristas* armed, unlike any other peasant movement, so that he could call on them for assistance whenever armed revolt broke out. Cedillo, much like a warlord, had virtually an independent power base, and he acted as a power broker between the central government and the local population. Ankerson quite rightly distinguishes him from other local *caudillos*, such as Emilio Portes Gil, Adalberto Tejeda, and Tomás Garrido Canabal, who based their power on political machines they organized at the state level but whose fate was more closely tied to their favorable relationship with the central government.

The second theme of the book treats the interrelationship between political stabilization and agrarian reform between 1920 and 1940. During the 1920s the agrarian bureaucracy administering the land reform program gradually replaced the patronage system developed by the individual military commanders and state governors and effectively assumed the functions of the prerevolutionary *hacendados*. This is not a new theme in regional histories, but Ankerson has documented extremely well how the central government promised land to armed *agraristas* in return for military service.

Ankerson has used local and national archives as well as U.S. and British diplomatic papers for his monograph, but his most notable research was done in the Secretaría de Reforma Agraria Archives (SRA). Since the earthquake in September makes it highly unlikely these archives will ever be used again, his findings are more significant. Yet to my mind he has still not provided sufficient information on the nature of the Cedillo movement. He claims that the Cedillo brothers led a *ranchero* revolt that sought retribution from the oppressive landlord class in 1913. This peasant jacquerie gradually radicalized its demands to include acquisition of land. But he bases his observations for this period almost entirely on the reports of the U.S. consul in the capital city of San Luis Potosí rather than on local sources.

It is only fair to compare this book with Romana Falcón's recently published *Revolución y caciquismo: San Luis Potosí, 1910–38*. Using almost identical sources except for the SRA archives, she stressed the overriding importance of the patronage system within Cedillo's movement. Both authors also agree that the Cárdenas administration viewed Cedillo as a threat to the consolidation of the postrevolutionary state. The two studies differ markedly on the populist nature of Cedillismo. Falcón maintains he was much more of a traditional *cacique* with strong ties to the landed and financial upper classes.

We can now place Ankerson's fine study alongside those of Franz Schryer and Ian Jacobs, who together have provided us with a new perspective on the prominent role played by the small farmer in the Mexican Revolution.

HEATHER FOWLER SALAMINI
Bradley University

W. GEORGE LOVELL. *Conquest and Survival in Colonial Guatemala: A Historical Geography of the Cuchumatán Highlands, 1500–1821*. Buffalo, N.Y.: McGill-Queen's University Press. 1985. Pp. xv, 254. $30.00.

This book is a valuable addition to the limited literature on colonial Guatemala. W. George Lovell, a historical geographer, studies the development of the region and peoples of the rugged Cuchumatán highland area of western Guatemala, west of the Rio Negro/Chixoy and north of Huehuetenango, up to 1821. The native peoples of the area spoke several languages of the Mayan stock.

The study is divided into three parts of quite unequal length. Following an introductory chapter, part 1 (chapter 2) treats the basic questions of physical geography and human adaptation to the environment, part 2 (chapters 3–5) studies preconquest native society, the initial phases of Spanish contact and conquest, and native population at the time of Spanish contact. Part 3 (chapters 6–9), by far the largest section, covers the period of Spanish colonial control from 1541 to 1821.

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Lovell was unable to find significant unpublished documentary material for the period before 1650. His estimates of population at contact, which he puts at 260,000, and his curve of population decline, therefore, have the same weaknesses as many others for the early colonial period. They have to be extrapolated from estimates made by colonial writers who had no particular qualifications as counters of population. Like many recent writers, Lovell uses as his model the high estimates of the preconquest population in Central Mexico developed by the Berkeley school of historical demography. A degree of hesitancy in accepting such high figures, however, seems to be called for in view of some detailed regional studies, such as that of William T. Sanders for Central Mexico (in *Native Population of the Americas in 1492* [1976], pp. 85–150) and that of Shirley Gorenstein and Helen Perlstein Pollard for the Pátzcuaro Basin. Lovell comes onto firmer documentary ground with his estimate of 47,000 as the population in 1578–82, establishing a low point of about 16,000 in 1683 and a recovery to 34,691 by 1825.

The strongest part of the book from a research point of view is for the period beginning with 1650. For this period, but particularly for the eighteenth century, his study of the economy, the relationship between the native highland villagers and the dominant society, and the hesitant trend of recuperation of the population gives valuable new insights into the efforts of an isolated society to maintain its physical existence and its cultural identity.

J. BENEDICT WARREN
University of Maryland

MICHAEL J. GONZALES. *Plantation Agriculture and Social Control in Northern Peru, 1875–1933*. (Latin American Monographs, number 62.) Austin: University of Texas Press. 1985. Pp. ix, 235.

In the last fifteen years the rise of a native Peruvian bourgeoisie to economic and political prominence during the second half of the nineteenth century has received increased attention from historians. Scholars such as Bill Albert, Heraclio Bonilla, Dennis Gilbert, Peter Klarén, and Ernesto Yepes del Castillo have all chronicled aspects of the path taken by a group of families, from commerce and the guano and coolie trades through investment in banking and in landowning on Peru's northern coast to political dominance in turn-of-the-century politics. Michael J. Gonzales's study, focusing specifically on the Aspíllaga family and their Cayaltí estate in the Saña Valley of the northern department of Lambayeque, is a contribution to this growing literature.

Gonzales relies mainly on the correspondence of the Aspíllaga family, made accessible to historians as a result of the Velasco regime's (1968–75) agrarian reform that expropriated the large sugar estates in the north of the country. This type of estate record is a rare and valuable window on the internal workings of agrarian enterprises. Supplementing the Aspíllaga records with secondary materials and other primary sources, Gonzales sets the history of Cayaltí in a regional context and carries the history of sugar from the colonial period into the twentieth century. His main focus, however, is the 1870–1933 period, during which a new elite of landowners became a major economic and political force at the national level. Gonzales explores the interrelated themes of investment strategies and labor control and provides us with a close-range view of changes in the sugar industry and their impact on Cayaltí. Among the positive results of such a perspective are an empirically rich understanding of the impact of plantation life on Chinese laborers between 1870 and 1900, a topic about which little has been written; a more detailed portrayal of the strategies used by planters to hire highland labor in the later 1890s and beyond; and a clear view of the Aspíllaga family's complex strategies for labor control that emerge in the politically turbulent decades of the early twentieth century.

Despite his heavy reliance on estate records, Gonzales ironically does not take advantage of all the potential methodological strengths of the hacienda study. Clearly conscious of the pitfalls involved in an overly myopic study of a single enterprise, he constantly tries to place his analysis within the context of the northern sugar industry as a whole. Unfortunately, this laudable effort tends to strand him between two conceptual levels. On the one hand, his chronicle of events at the regional and national levels is based on published sources and does not provide us with a new perspective. On the other hand, because his treatment of these levels constitutes a sizable portion of his study, he is then unable to explore the local sources fully.

Gonzales's analysis of Cayaltí and the Aspíllagas often raises as many questions as it answers. He notes, for example, that the Aspíllagas controlled local political officials, yet he does not examine the relationship in depth through the family's own correspondence or through local municipal and provincial documents. He also mentions Antero Aspíllaga's unsuccessful bid for the presidency in 1917 and even ties it to the problems with increased worker militancy, but we do not get a clear image of the event's importance as a moment of ideological and political crisis for the family. Furthermore, even though the family's correspondence seems particularly rich on issues such as labor relations, the ideological and economic contradictions inherent in

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the relationship between the family and its work force are noted rather than explored. Finally, the author does not adequately explain the rationale behind changing investment decisions within the enterprise. Why did the Aspíllagas decide to invest less than other planters as the twentieth century progressed? Why did they remain on the estate, administering it personally after most planters had become absentee landowners? Why did they suddenly become absentee landowners in the 1940s? What was the relationship between Cayaltí and Palto, their cotton estate in the south?

In the end, one is left wishing that the author had spent more time exploring the case of Cayaltí itself, perhaps by more consistently supplementing hacienda records with local political, judicial, and newspaper sources. The sections that probe the richness of estate life and relationships are the best in the book; they give a taste for the abundance of implication a closer look could have yielded. And, although it is always important to look at local events in a regional and national context, the hacienda study can make its greatest contribution in presenting the variegated texture of local ideological, economic, cultural, and social relationships.

FLORENCIA E. MALLON
University of Wisconsin

GEORGE BRIZAN. *Grenada: Island of Conflict; From Amerindians to People's Revolution, 1498–1979.* (Third World Studies.) London: Zed; distributed by Biblio Distribution Center, Totowa, N.J. 1984. Pp. xvii, 381. Cloth $35.50, paper $13.95.

The rapidity with which Grenada was thrust into world view because of the bloody debacle of 1983 has resulted in an outpouring of literature of varying quality on the tiny island-nation. Although most of these works have concentrated on the post-1979 period, George Brizan, at present a minister in the Grenada government, has used local and international archival sources to provide us with a detailed history of the island from the pre-Columbian era to the people's revolution in 1979.

Brizan perceives conflict as a common thread running throughout the island's history. Although the national interests of the major European powers formed the basis of conflicts in the seventeenth and early eighteenth centuries, race and creed were the basic ingredients up to 1838 when plantation slavery formally ended. Thereafter, color, class, and wealth proved to be major contributors to discord. The author skillfully discusses the unceasing efforts of landless blacks to improve their socioeconomic status from the mid-nineteenth century onwards. Yet, because political power remained in the hands of the whites and the "colored" middle class, black laborers were denied meaningful participation in the island's major institutions until 1951, when trade unionist Eric Gairy led the first political and social revolution.

Ironically, Gairy the hero soon became anathema to the growing population of the island, more than 60 percent of whom were under age twenty-five by 1970. Opposition increased to his calls for independence, dubbed by his detractors as a "flag and anthem independence, full of trappings but without substance for the people and their ills" (p. 337). His ill-advised decision to initiate a series of repressive acts against his opponents, argues Brizan, saw Gairy unconsciously laying the groundwork for the 1979 revolution by providing Maurice Bishop with a youthful cadre of enthusiastic supporters.

The book is not without its shortcomings. By emphasizing the elements of conflict and strife, the author fails to appreciate adequately the creative forces at work within the society. We are accordingly left with a myopic, almost pathological, view of society during the entire period, when, apparently, nothing worthwhile or creative emerged. I doubt Grenadians or other historians would find this picture, which the author has perhaps unwittingly painted, to be an accurate reflection of the past. Amid all the apparent political chaos can be found the embryonic strivings of a people for identity and expression, which have been accorded an all-too-scanty treatment. Edward Brathwaite's 1971 manuscript on Creolization in Jamaica, which Brizan does not cite, is clearly instructive for any treatment of the evolution of modern Caribbean societies. Such a comparative framework also would have allowed Brizan to place migration within its proper context as an agent of political and social change in the twentieth century.

This, of course, is probably asking too much of an author who has provided us with the most comprehensive general history to date of the island. Those who seek to understand the background to contemporary Grenadian economy and political culture will find this book indispensable.

EDWARD L. COX
University of South Carolina

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

RICHARD LANGHORNE, editor. *Diplomacy and Intelligence during the Second World War: Essays in Honour of F. H. Hinsley*. New York: Cambridge University Press. 1985. Pp. vii, 329. $39.50.

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ARTICLES

TO THE EDITOR:

In his article "Vagrants, Beggars, and Bandits: Social Origins of Cuban Separatism, 1878–1895," Louis A. Pérez, Jr., fails to convince us of his argument that Cuban bandits conformed to Eric Hobsbawm's model of social banditry (*AHR*, 90 [1985]: 1092–1121). The most tenacious bandits operated in Havana province, and that area did not fit the cause-and-effect analysis that Professor Pérez presents. Many of the well-known bandits Pérez names operated in the Havana countryside and were not displaced farmers at all; in addition, commercial agriculture was not new to the area.

Although Spanish authorities bragged in 1889 that brigands had been suppressed in the rest of the island, they admitted that banditry continued to flourish in the area that stretched from the capital city of Havana to the southern port of Batabanó and from the fringes of Pinar del Río province on the west to the edge of Matanzas on the east. For one thing, the topography suited bandit activity. South of Havana, highland areas crossed from west to east in the shape of an "x," with well-watered valleys suitable for cattle, fruit trees, sugar, and other crops. South of the highlands, the western end of the great, flat sugar plain terminated in the tobacco vegas of the Vuelta Abajo. The densely populated highland areas were full of caves; the agricultural areas in the south were interspersed with woodlands. Closer to the coast, the land became swampy, and coastal woodlands—home to charcoal makers—made an excellent hiding place. Given the small size of Havana province, its dense population, abundance of hiding places, and supportive population, hit-and-run tactics paid off.

This same topography fostered the variety of agricultural production that characterized the area, with many locations more suitable to small-scale cultivation than to large plantations. Havana province had experienced agrarian transitions for decades before 1878, and its agriculture was distinctly commercial. Havana farmers grew coffee and tobacco as well as sugar for export. They produced food crops and raised cows and pigs for the domestic market and draft animals for the sugar industry.

A decrease in the number of sugar mills and coffee plantations took place in the Güines area (southeast of the city of Havana) at midcentury, but it was accompanied by an increase in the number of holdings devoted to cattle raising and small-scale food production. This development followed the opening of Cuba's first railroad, which connected the capital and Güines in 1837. Produce was shipped from areas all along the railroad as early as 1840. Additional lines connected Havana with Batabanó on the southern coast and San Antonio de los Baños on the west, forming an irregular circle through the heart of the province. Side by side with sugar producers, small farmers grew potatoes, vegetables, rice, fruit, and grains for the large rural enterprises and the urban market.

Post-Zanjón agricultural reconstruction in the area benefited tobacco growers and cattle raisers. Many owners of small sugar estates turned them into cattle farms or rented the land to farmers who raised animals and crops. Farmers grew cane alongside their other crops to take advantage of the expanding market for sugar, or they became *colonos* and grew cane exclusively on land rented from absentee landlords or from the large sugar producers (*centrales*). In the densely populated province, *centrales* competed for cane, which could be sold to more than one mill, and the *colonos* could demand a better price for their crops.

The people who supported the Havana bandits were generally described as small farmers (*sitieros*),

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colonos, or owners and renters of farms with pasturage for cattle (*potreros*). They were often relatives and friends who had lived in the same location for at least a generation, not an uprooted or displaced group. They were, however, *cubanos* whose experience included the struggle of the Ten Years' War against Spaniards. In short, the bandits emerged from a rural society that was already politically conscious and nationalistic—hardly Hobsbawm's prepolitical rebels. There is evidence that Manuel García's boyhood friends, men with land and social standing, recruited García into the insurrectionist ranks when he got into trouble with the law.

If the bandits of Havana were not peasants displaced by expanding latifundia, why did they become outlaws? They did so for a variety of reasons: they were on the run from authorities, sought adventure or escape from boredom, or were perhaps spurred by entrepreneurial ambitions. Many of the Havana bandits became criminals when they stole cattle. The city of Havana counted some 125 butcher shops in 1887, and the smaller provincial towns had their own retail outlets. Manuel García became a cattle thief as a young man, but he was over thirty when he became a bandit. Manuel's brother Vicente, a member of García's gang, was remembered fondly by a small-town butcher to whom he supplied cattle. Arturo García, no relation to Manuel but nephew of the bandit Carlos García, was the son of a fisherman born in the town of Artemisa in the heart of the tobacco region. Considered a daring child and a terror among the boys in the town, Arturo became a cattle thief as a teenager, progressing to kidnapping, robbery, and murder before he was shot down while in his twenties.

Other Havana-based bandits owned land. Sixto Valera, who died in an ambush intended to kill Manuel García, was a popular landowner of la Catalina and had attended the *colegio* in Havana. When Rafael Plasencia quit the outlaw life in 1891, he retired to land he inherited from his grandparents. Andrés Santana, one of the bloodiest bandits of García's gang, was the son of honest, hard-working immigrants from the Canary Islands who had prospered working the land. Four of their six children lived near them and farmed; Andrés and one brother were bandits.

Pedro Palenzuela and Eulogio Rivero first hunted Manuel García on behalf of the Spaniards and then joined García's gang. Palenzuela was from the southern coast and was more familiar with the sloops and schooners of Batabanó than with plow and oxen. Rivero's brother worked a *colonía* for the sugar mill Sastre, where the bandits often hid.

The Havana bandits served the revolutionary cause and their own pockets, collecting money from rich and poor alike, Spaniard and Cuban. Their connections crossed class lines, and they were capable of bloody reprisals against peasants who betrayed them. Although José Martí appointed Manuel García to a leadership position in his insurrectionary movement, he rejected García's financial contributions because of the taint of criminality. To give the bandits their fair share of the revolutionary spotlight requires more information, which I intend to present in a book I am writing on the lawless liberators.

ROSALIE SCHWARTZ
San Diego, California

PROFESSOR PÉREZ REPLIES:

We appear to be in substantial agreement on a number of points, to wit, that the local topography (highlands, woods, caves, and swamps) "suited bandit activity"; that bandits were beneficiary to "a supportive population"; that bandits were capable of "bloody reprisals" against peasants who betrayed them.

My answer to those points of disagreement is that Rosalie Schwartz is correct in her observation that the number of sugar mills declined in many districts of post-Zanjón Cuba. Their demise did not, however, signal new opportunities for small nonsugar agriculturists. On the contrary, although the number of mills declined, the actual acreage of land devoted to sugar increased dramatically—to the detriment of other agricultural products and other land tenure forms. Scores of farmers, peasants, and rural workers fell on hard times. Outlawry offered the opportunity both for release and for revenge.

Dr. Schwartz, quite remarkably, appears ready to accept government public pronouncements at face value. She confuses Spanish rhetoric with the Cuban reality. Spanish authorities always "bragged" that they had suppressed banditry outside Havana: this was the official government position. The reality was quite different. An examination of the local records and private correspondence from provincial authorities—as compared to the public pronouncements by the central government—as well as a study of the field reports filed by *Guardia Civil* officers in the interior and even a cursory reading of the provincial press suggest that conditions in the outlying *municipios* did not improve. Vast regions of the interior were as beset by banditry in the 1890s as they had been in the 1880s. Schwartz's contention that bandits and their supporters emerged from a rural society that was "already politically conscious and nationalistic" is untenable. All of the evidence suggests the exact opposite. These rural regions were unresponsive if not hostile to the patriotic summons. When these districts were integrated into the revolutionary process a decade later, it was

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achieved from the outside, and it was in response largely to social grievances, not political ones, in pursuit of local changes, not national ones. Schwartz seems to be saying that "bandits became criminals" for the hell of it: they were bored and wanted adventure. That the condition that plunged rural Cuba into two decades of social turmoil led to several states of siege and engaged an estimated 25,000 Spanish soldiers was the result of the mischief of a handful of bored rural men strains credulity. Given the number and frequency of abortive uprisings in the name of Cuba Libre between 1878 and 1895, Cubans seeking relief from boredom and looking for adventure—especially if they were "politically conscious and nationalistic"—had available any number of opportunities to occupy their free time.

Cuban bandits were a mixed lot—of course. Their motives were neither simple nor pure. They pursued mixed goals with uneven success. There are obviously other vantage points from which to approach post-Zanjón banditry in Cuba. As Schwartz's communication demonstrates so well, bandits lend themselves nicely to the anecdotal: ambush and assassination, revenge and reprisal, cattle rustling, extortion, and abduction—all in all, plots deep with intrigue and adventure. I will look forward to reading the findings of Schwartz's research in the book she intends to write.

LOUIS A. PÉREZ, JR.
University of South Florida

TO THE EDITOR:

In his *Research Note*, "Stature and Nutrition in the Habsburg Monarchy" (*AHR*, 90 [1985]: 1149–61), John Komlos finds that the last regions of the Habsburg monarchy to become market oriented actually experienced a decline in nutritional status when they entered the market. Komlos says, "this finding conforms to the economic theories that stress the initial disadvantages of free trade to the less developed partner" (p. 1159).

But how can any conclusions be drawn about the net profit or loss that a region derives from trade when the researcher has measured only one of the two items being traded—in this case, the item (food) being traded away?

Komlos also does not address the possibility that free trade may have led to population growth in the more slowly developing regions, just as such growth preceded the rise of the market in the regions that developed more quickly.

RICHARD JOFFE
Misawa Airbase, Japan

PROFESSOR KOMLOS REPLIES:

I appreciate the interest in my article. I should point out that I did not intend to create the impression that either Hungary or the Polish provinces derived a "net profit or loss" from trade with Austria and Bohemia in the late eighteenth century. In fact, I intimated that, considered as a whole, they might have gained from the trade arrangements. I only cautioned that one should not overlook the fact that there were some losers as well, namely, the landless and the urban wage earners whose nominal income did not keep pace with the sudden and persistent rise in grain prices. I wrote, "Even if the society gains from free trade, the rapid economic change that results from it imposes hardship as well. Although the gains might be greater than the burdens, especially in the long run, the costs and benefits are not distributed evenly among various segments of the population. The less advantageously placed members of the society bear the brunt of the burdens of economic development" (p. 1159). Hence, I did not draw inferences about the net gains from trade because, for one thing, we do not know how many people were in the gainers' and how many in the losers' category. Even if we did know, the calculation could not be made unless some further assumptions were made about the character of the individuals involved. The gainers might have been few, but their gains could have meant a lot to them, whereas the losers—though many—could have been somewhat indifferent to their losses. It would therefore be rather difficult to add up the changes in the well-being of all of the individuals. This problem could be circumvented if one were to assume that the gainers and the losers were identical; that is, one could suppose that a given amount of consumption generated the same amount of satisfaction for all. This assumption would pose still other difficulties because a one-unit increase in consumption (even for the same individual) does not necessarily increase satisfaction to the same extent as a one-unit decrease in consumption detracts from it. Therefore, one would have to make further assumptions on the functional relationship between consumption and satisfaction. This problem is thus intractable, and I did not care to speculate on it.

I did, however, find evidence that the average height of the Hungarian and Polish male populations was declining, and I interpreted this to indicate a fall in nutritional status. That is all I asserted: "economic development can lead to a decline in nutrition for part of a society undergoing rapid economic change" (p. 1159). This should not be difficult to accept, for I did not rule out the possibility that, notwithstanding the decline in nutritional status, well-being increased. Consider the following.

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Suppose initially a peasant produces 100 units of grain and consumes all of it himself. Suppose, moreover, that during the following period a merchant appears and offers him twenty yards of linen cloth for twenty units of grain, and he accepts the trade. He will be clothed better but eat less. His nutritional status would no doubt decline, although he might be completely satisfied with the exchange and consider himself as well off as, or perhaps better off than, he was before. There is nothing confounding about it. For purposes of this analysis, it hardly matters whether he received twenty yards of linen or ten. Since the exchange is assumed to be voluntary, the peasant considers himself better off at the moment the exchange takes place. Hence, I did not dwell on the quantity of industrial imports into Hungary. It made no difference to the point I wished to make. To be sure, the peasant could regret the exchange after it occurred. Because of unanticipated weather conditions, his subsequent harvest could have been below expectations. He or members of his family could suffer, and his subjectively measured well-being could also decline. The problem is even more complex than this. Population growth raised the marginal product of land, and that could have induced landlords to raise rents. The peasant in the example above might have lost twenty units of grain because of increased rents. The landlord could have been the one who exchanged the grain for the linen cloth, thereby benefiting from the opportunity to do so. He gained, but the peasant lost. In short, theory is not an effective guide in unraveling the impact of trade on well-being in this region of the world at the end of the eighteenth century. All one can assert is that the problem is complex and that further data are needed to attempt a fuller answer than the one I gave in the article.

I did not raise the question of the effect of trade on population growth because, in the absence of evidence, one could not do more than speculate about it. One might well assume that the birth rate was a function of well-being and that those who benefited from trade might marry earlier and have more children, whereas those who lost from the new market arrangement had fewer children. Again, since we do not know the relevant number of individuals in each category, we can hardly deduce the net effect. Of course, the issue could be more complicated than this because those who gained from trade could be beyond childbearing age, whereas those who lost might be the younger members of society. Then population growth would decline. We could also suppose that those on the upper end of the income distribution were the ones who gained from trade and that, for them, financial considerations might not have posed an effective constraint on the rate at which they were procreating. Thus, their crude birth rate might not be sensitive to the gains generated by trade, whereas, if the poor became poorer, their family formation might be postponed and, consequently, their birth rate could decline. The net effect of market integration might have been a decline in the rate of population growth. We could make similar suppositions in regard to the death rate. Consider the abovementioned peasant experiencing unanticipated adverse weather. Grain consumption of some members of his family might have declined sufficiently that they became susceptible to a fatal disease. This might not have happened if, in the absence of a market for grain, he had kept the twenty units of grain in stock. Hence, the effect of trade on population growth is not easily ascertained in this instance without a considerable expansion of our knowledge of the pertinent facts. In sum, the issues raised are quite complex, and, with the available evidence, I believe one can hardly compose a fuller analysis than the one I presented in my article.

JOHN KOMLOS
University of North Carolina

REVIEWS OF BOOKS

TO THE EDITOR:

Mary Frances Berry seems to ignore serious shortcomings in Gilbert Ware's *William Hastie: Grace under Pressure* (*AHR*, 90 [1985]: 1292–93). Someone should point out that this work is badly written and disorganized and shows an unfortunate tendency to slip into slang and cliché, for example, "discombobulate," "someone snitched," "a feather in Stimpson's cap," "Tuskegee was a thorn in Hastie's side," and "James G. Thompson started all the jive." (And who was James G. Thompson? There is no identification in the text and no reference in the index.)

Professor Ware has trouble with terminological exactitude as well. He persists in referring to the "Air Corps" well after that service became the U.S. Army Air Force and writes of "Judge Harold R. Medina's court in Foley Square in Manhattan." But what type of court? After all, this is a biography of a legal figure.

Factual errors abound. Referring to black officers, Professor Ware claims that "the War Department had never collected data about their performance in World War I." Yet the U.S. Army War College conducted no less than ten studies dealing at least in part with just that subject. The most significant of these efforts were Training Course #18, "The Organization and Training of Negroes" (1920), and "The Use of Negro Manpower in War" (1925). The

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conclusions were undoubtedly wrong-headed, but the studies were made. And Professor Ware repeats the legend that the pioneer all-black 332nd Fighter Group never lost a bomber. Squadron records document that even this excellent unit lost at least two. The author has the 332nd flying a combat mission over Berlin four months after the German surrender and Congressman Vito Marcantonio sending a memo to Judge Hastie "on 17 September, 1945 for comment before presenting it to President Roosevelt"—not likely, because FDR died on April 12, 1945.

Finally, Professor Ware makes little attempt to suppress his passionate commitment and is thereby led into ahistorical attitudes and judgments. When the National Press Club, for example, refuses to serve Judge Hastie, that institution becomes "dedicated to the proposition that all black men were created unequal to all white men, regardless of achievement" (p. 163). And on page 126 Professor Ware refers to the "segregation that contaminated the other military services."

We have come to expect better things from Oxford University Press; certainly Judge Hastie deserved better.

STANLEY SANDLER
Department of the Army
Fort Bragg, North Carolina

PROFESSOR WARE REPLIES:

Some of Mr. Sandler's comments are useful; none are evidence of "serious shortcomings" in a book whose thesis he resents but cannot rebut.

Actually, the salient feature of the letter is Mr. Sandler's attempt to distract readers from the substance of the book. Indeed, he misses the entire point of the biography. Perhaps this is reason enough for me to state it here: William Hastie, a black man, distinguished himself in the bittersweet war against bigotry.

Did Mr. Sandler write to criticize or to censure? He disparages my "passionate commitment" to (I assume) Hastie's war. But this just proves that you never can tell when someone will pay you a compliment.

GILBERT WARE
Drexel University

PROFESSOR BERRY REPLIES:

I stand by my review.

MARY FRANCES BERRY
Howard University

NOTE

Because of an *AHR* editorial policy, Professor Cienciala is not able to reply to Professor Stephen Blanke's letter in their exchange (91 [1986]: 242–43). Professor Cienciala nevertheless wishes to inform interested readers that she is well aware of the Polish-speaking Upper Silesian vote in 1921 and that she disagrees with Professor Blanke's view that it should be respected as their "choice."

THE EDITOR

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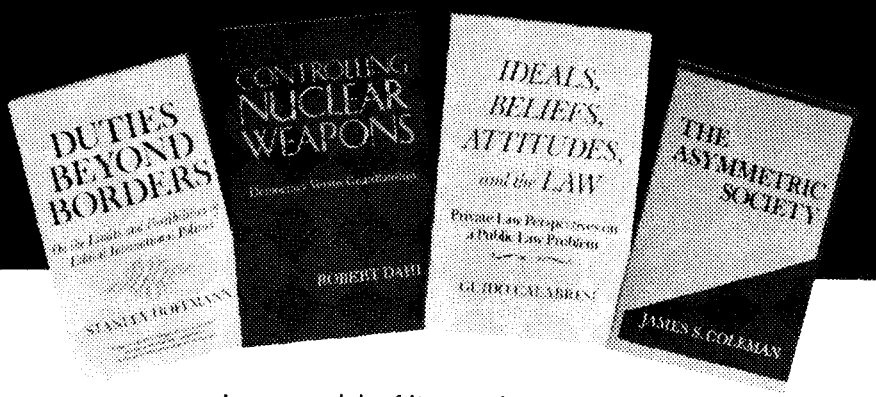
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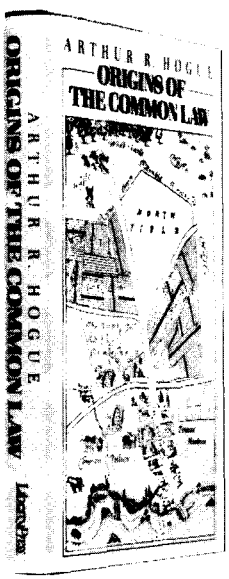


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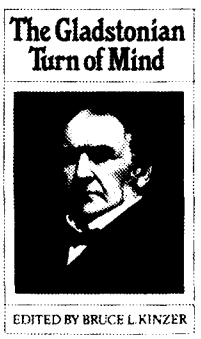
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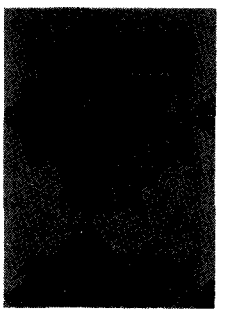
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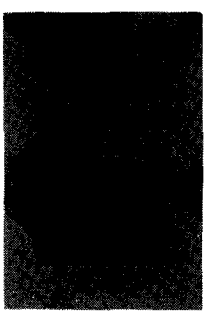


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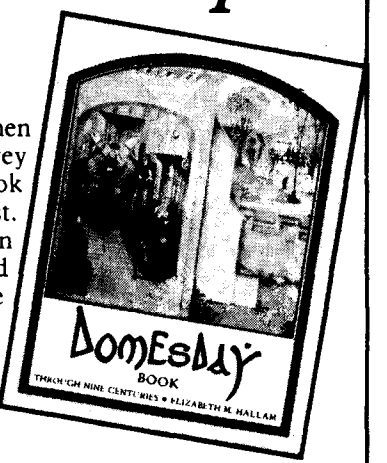
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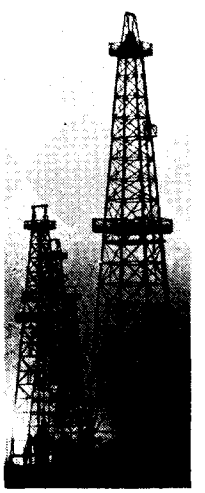


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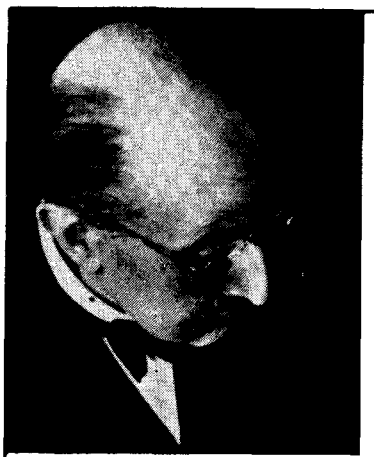
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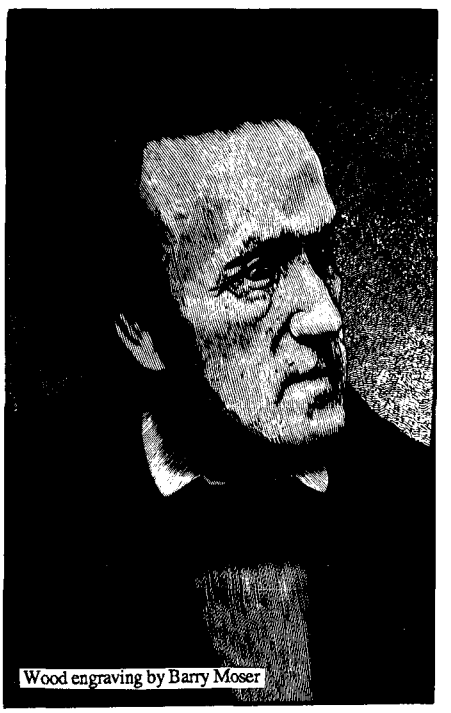
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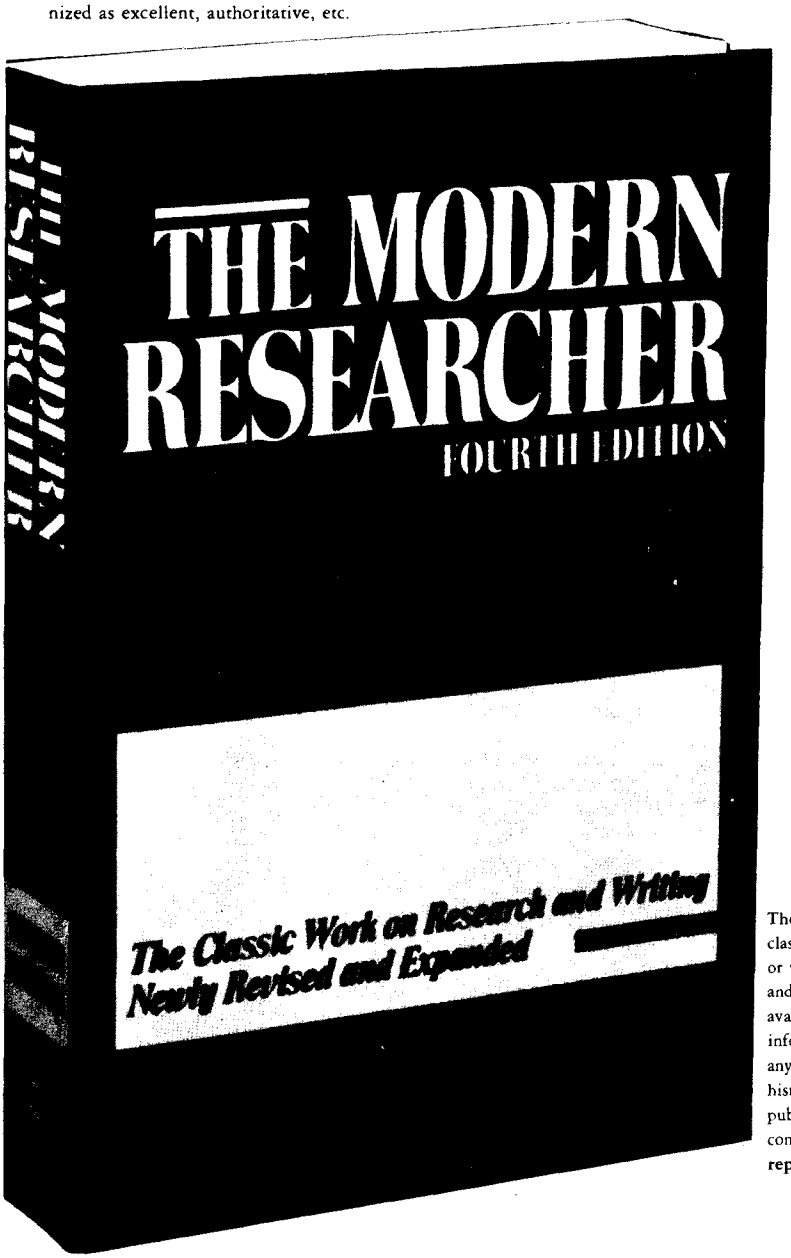
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